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ABSTRACT TERMS

I

NOUNS like "humanity," "yellowness," "piety," and "virtue" have often been analyzed by logicians and philosophers as names of abstract entities and labeled "abstract terms," while the corresponding adjectives or nouns "human," "man," "yellow," "pious," and "virtuous" have been taken as naming concrete entities and labeled "concrete terms." In more recent forms of this analysis expressions are called "names" only when they purport to apply to one and only one object, so that concrete terms like the aforementioned are distinguished from names and called "concrete general terms" to indicate that each may apply to many concrete entities. General terms, moreover, as opposed to names, are said to characterize (to signify something true of) and not merely to designate or denote the objects they apply to, so that even if a general term should apply to only one object it would still not be a name because it would characterize the object. But then abstract terms remain names on both counts. They purport to apply to one and only one object (a single abstract entity) and to denote that object without characterizing it. They have thus been relabeled "abstract singular terms." Since it seems we can often (if not always) say what we want to say by substituting sentences containing concrete general terms for sentences containing abstract singular terms, it has been held that in so far as we can avoid using terms of the latter sort we need not admit the existence of the entities they purport to name.

I believe this analysis is fundamentally mistaken and needs to be altered in a number of respects. For one thing, it includes under the one heading "abstract singular terms" words with very different functions and fails to distinguish them. I want to consider some of these different functions and in the process to present some suggestions for the changes which I think the analysis requires.

II

I am going to begin with a group of words which are generally not given as examples of abstract singular terms, but which are close to such terms on various points of grammar and syntax. I mean the group of words which might be labeled (when the use of "name" is not restricted) "names of materials," names like "wood," "soil," "iron," "steel," and names of chemical elements. These words generally do not take singular and plural forms in the way that concrete nouns such as the names of plants and animals do. We say that oxygen is necessary for life and that steel revolutionized the building industry, just as we say that piety is dear to the gods and that virtue is its own reward. The definite article is not required in any of these cases as it is when we use a concrete noun in a similar construction, such as "The lion is king of beasts" or "The Japanese beetle is a great destroyer of crops." Names of materials have uses in the plural which parallel those of abstract rather than concrete nouns. "Woods," "soils," and "steels" are frequently used in speaking of different kinds or grades of wood, soil, and steel, just as "virtues" and "sciences" are used in speaking of different kinds or grades of virtue and science.

If we wish to speak of concrete individuals, we use individualizing expressions with names of materials as we do with abstract terms. We say that something is a piece, bit, chip, pile, heap, volume, rod, length, or compound, and then add "of" followed by the name or names of appropriate materials, just as we say that something is an act or application of piety, virtue, or knowledge. But we do not use these individualizing expressions with concrete nouns. A tree, lion, or beetle is simply an individual tree, lion, or beetle, unless it is clear from the context that a kind or variety of one of these is intended. In no case would the individual be a piece, bit, act, or application of tree, lion, or beetle. On the other hand, a wood, soil, or steel, like a virtue or knowledge, would be a kind, grade, or variety unless the context made it clear that an ellipsis was being used with some individualizing expression to be understood. Finally, names of materials, like abstract terms, have corresponding

adjectival forms. Usually with names of materials we use the same word as noun or adjective, and say "a steel rod" or "a rod of steel," "a carbon compound," or "a compound containing carbon"; but occasionally we have a separate form for the adjective, as we say "a wooden chair" or "a chair of wood" just as we say "a virtuous man" or "a man of virtue."

I do not propose to dwell further on the similarities between the grammar of names of materials and that of terms generally regarded as abstract. We could of course discover peculiarities which would distinguish some terms in the one group from any of those in the other, as well as further points of similarity, but I believe I have already said enough for the points I want to make about abstract terms. I want now to suggest an explanation for the similarities, or, in other words, to speculate about a deep or philosophic grammar underlying the surface grammar.

III

The fact that a thing is made of or consists of a certain material is generally regarded as a fact about or as a condition of the thing. This is most obvious in those cases where we say that the thing could be made of several different materials and still be the same thing, or at least the same kind of thing. But even when we think that something can be of only one material, we still regard the specification of this material as stating a fact about, a characterization of, or condition of the thing. We thus come to think of materials as conditions of things rather than simply as things in their own right, so that when one speaks of wood, steel, or carbon simply as things and not as conditions of things there is a sense in which we may say he is speaking abstractly. He is talking about the materials in abstraction or separation from the numerous individual things of which they are conditions. In these circumstances, then, the grammar of names of materials becomes that of abstract terms.

There are exceptions to this when the material itself is something characterized by a fairly definite size and shape, or something with which we usually associate certain activities, ends,

or values. Brick, rock, stone, wood, and steel are all building materials, yet brick alone of these is characterized simply as material by size and shape. "A brick," as opposed to "a wood" or "a steel," thus usually means a concrete individual rather than a kind or variety, and the grammar of "brick" is generally that of a concrete noun, even though we say "a house of brick." Rocks and stones, as opposed to the others, are often regarded as objects which can be moved, pushed, or thrown, and used for specific purposes, as milestones or gravestones. The grammar of "rock" and "stone" is thus similar to that of "brick" rather than to that of "wood" or "steel," although names of kinds of rock or stone, like "granite" and "limestone," are used more in the pattern of abstract terms. Names of precious stones, on the other hand, generally have the grammar of concrete nouns, though if we attached no more value to diamonds and rubies than we do to bits of granite and limestone, we might use "a diamond" and "a ruby" as we use "a granite" and "a limestone."

These exceptions indicate that the abstract and concrete grammar of names of materials is determined in part, at least, by the way we happen to characterize things and by our activities and interests. There is nothing to indicate that it is determined by our assumptions concerning the existence of abstract entities. We hardly want to claim that in saying "a wooden chair" rather than "a chair of wood" one avoids the need of assuming an abstract entity. But why, then, has it been maintained that in saying "a virtuous man" rather than "a man of virtue" one does avoid such a need?

An apparent answer is that the grammatical similarity between "of wood" and "of virtue" is purely on the surface, and that the two phrases are easily seen to be grammatically quite different at a deeper level. The "of" in the first case means "physically composed of" and always takes as its object an expression which applies to the same concrete individual as the one to which the whole phrase applies. That is, both "wood" and "of wood" apply to an individual chair; we say simply that a chair is wood. The adjective "wooden," then, is merely a stylistic alternative for "of wood" and introduces no change in the presupposition of entities, since "wood" and "wooden" both apply to the same

individual. The "of" in the second case, on the other hand, means having a certain quality, and always takes as its object an expression which applies to an entity other than the one to which the whole phrase applies. We do not say a man is virtue but only that he is a man of virtue. The adjective "virtuous," then, is not merely a stylistic alternative for "of virtue," and it introduces a change in the presupposition of entities since "virtuous" and "virtue" do not apply to the same entities.

Yet this analysis overlooks the fact that while we say "This chair is of wood" we do not say "This man is of virtue." We say rather "This man is a man of virtue." The analysis, then, makes the mistake of taking the phrases "of wood" and "of virtue" as those to be contrasted, whereas the proper phrases are the longer ones, "a chair of wood" and "a man of virtue." We can say that these two phrases mean, respectively, a chair having wood as its material and a man having virtue as one of his qualities. This interpretation brings out the point that both phrases are used to signify a concrete individual as having a certain condition. The conditions, to be sure, are different and their differences are reflected in various points of grammar, but they are not the sort of differences which introduce a change in the presupposition of entities. We say neither that a chair *is* wood as its material nor that a man *is* virtue as one of his qualities, and if we claim that we assume another entity besides the individual man when we say that he *has* virtue as one of his qualities rather than *is* virtuous, we must claim that we assume another entity besides the individual chair when we say that it *has* wood as its material rather than *is* wooden. The fact that we say, "This chair is wood or of wood" but not "This man is virtue or of virtue," is the result of a difference in the conditions of having wood as a material and virtue as a quality, and does not provide an argument for maintaining that we assume an extra entity when we use "virtue" rather than "virtuous." We might just as well argue for the extra entity in the case of "wood" and not "virtue" on the ground that we say a chair's wood exists before it exists (and thereby assume the wood to be a separate entity), though we do not say a man's virtue exists before he exists. With either argument we are equally misled by surface grammar.

I do not want to suggest that these speculations are enough to establish that the presupposition of entities in the use of "virtue" and "wood" is always the same. In fact, I do not think it is always the same, but we have yet to consider that use of abstract terms which will bring out the difference.

IV

Names of colors are commonly given as examples of abstract terms, but it is not so common to distinguish between a use of the adjective as a noun and a use of the noun formed from the adjective by the addition of the suffix "-ness." The distinction I have in mind may be illustrated as follows. Suppose that we are speaking of a wound that is healing and losing its inflammation. We might say, "The redness is disappearing," "The red color is disappearing," or perhaps even, "The red is disappearing." But "The color red is disappearing" will not do, since the phrase "the color red" applies only when we wish to speak of the color itself and not of the color as a condition or symptom of something else. When we want simply to compare colors and we say "Red is warmer than yellow," we will accept "The color red is warmer than the color yellow" as an expanded and more pedantic way of saying what we want to say. But "Redness is warmer than yellowness" is strictly not what we want. An expansion of this sentence would be, "The quality of being red is warmer than the quality of being yellow," which hardly makes sense. A proper use of the ness-forms would be, "The yellowness which is characteristic of jaundice is more shocking than the redness which is characteristic of inflammation." When expanded as before this becomes, "The quality of being yellow which is characteristic of jaundice is more shocking than the quality of being red which is characteristic of inflammation," which, however cumbersome, makes perfectly good sense. We might prefer the shortened form, "the yellow of jaundice," but this would be understood as expandable into "the yellow color which is characteristic of jaundice." It could not be understood as expandable into "the color yellow, which is characteristic of jaundice." The color yellow is not characteristic

of anything, though it is characteristic of it that it is less warm than the color red. What is characteristic of jaundice is a yellow color, a yellowness, or a quality of being yellow, but not the color yellow.

This difference in the use of "the color yellow" and "yellowness" is philosophically of fundamental importance, since it provides the occasion for questions about the existence of entities. When we say, "This chair has yellowness," we may claim that we are no more presupposing a further entity besides the chair than we are when we say, "This chair has wood as its material." While the sentences, "Look at the yellowness of that chair!" and "Look at that chair!" seem to command us to look at different things, we do not have to say that because we think we can obey the commands and either look at the yellowness without looking at the chair, or look at the chair without (specifically) looking at its yellowness, that therefore we are presupposing two separate entities. We are perfectly justified in claiming that in looking only at the yellowness we are looking at something inseparable in existence from the chair and hence not at another entity besides the chair. To be sure, the chair is not inseparable in existence from its yellowness, since it may change its color and still exist, but this does not make the yellowness a separate entity which can exist when the chair does not have it. The situation in a way is reversed in the case of a chair and its wood, for the chair cannot exist without its wood though the latter existed first and may continue to exist when the former has ceased to exist. Yet "wood" rather than "chair" remains the grammatical analogue of "yellowness" because we can look at the wood as we can the yellowness without looking at the chair, while in looking at the chair we are also looking at its yellowness and its wood, only not specifically. It is with this grammatical point in mind that we say the wood is not a further entity besides the chair, although in saying this we do not mean that the wood must cease to exist when the chair does. We mean only that while the wooden chair exists, there is but one entity and not two, the chair and its wood; just as while the yellow chair exists, there is but one entity and not two, the chair and its yellowness.

"Yellowness," then, is a term we use when we wish to speak about or call attention to the yellow color of a concrete individual —its quality of being yellow. A similar remark holds for a great many abstract terms, and not merely for those formed from color words by adding the suffix "-ness." We talk about and call people's attention to looks, shapes, textures, materials, tastes, smells, and sounds, all as qualities of things, and in doing so we use terms grammatically similar to "yellowness." We also speak in the same way of a person's or thing's relations, dispositions, traits of character, and habits. In all of these cases we imply a separation between a thing or things and that feature of a thing or things which is to be paid attention to and considered. But this is always a separation to be realized only in an act of attention and not in the separate existence of two or more entities. When we say, "Consider his relation to his father," we are not presupposing three separately existing entities, the father, the son, and the relation between them; we are presupposing only that one can pay attention to the relation without paying attention to the father or the son as he would in response to "Consider the father" or "Consider the son."

I shall call this use of abstract terms *directive abstraction*, since it directs attention but does not presuppose a separation in existence. Directive abstraction is often confused with another use of abstract terms, a use which raises further questions about the existence of entities. We must consider briefly this second kind of abstraction before we return to our question about the use of "virtue" and "wood."

V

Let us begin with a simplified version of Hume's different shades of blue arranged in a series "from the deepest to the lightest." I am not interested in Hume's psychological question of whether a person can be led to form an idea of a color he has never seen; I am interested rather in how we use the verb "to be" in talking about members of the series. Suppose that the series as represented contains only four shades and that the second is visibly lighter than the first in the same degree as the

third is lighter than the second, but that the fourth is obviously lighter than the third by a greater degree. In looking at this arrangement and assuming that each member after the first was supposed to be lighter than its predecessor by the same degree, one may be expected to comment, "There is at least one more shade which belongs between the third and fourth." Now in what sense is this person using the phrase "there is"?

Clearly, he is not using it in the sense of, "There exists somewhere in the world a concrete individual having a shade of blue which belongs between the third and fourth." The existence of such an individual is quite irrelevant. All that is relevant is the given representation of the series, that is, the spots of ink in a row on a sheet of paper, plus the assumption that these spots or patches are supposed to be ordered in a certain way. It is not relevant whether there exists a chemical with the aid of which we could make an ink that would produce on the paper a shade of blue which belongs between the third and fourth shades. In saying that there is this intermediate shade one does not mean that such a chemical exists. He means only that given a certain way of ordering shades of blue in a series, there is at least one more member to be included between what are the third and fourth members in a particular representation of this series. His use of "there is" is thus determined solely by his application of a principle of order to the row of color patches he sees on the paper. That there is this missing member in the row of patches follows logically from the assumption that the row represents shades of blue in a given serial order. "There is" in this case marks the result of a logical deduction, a result which logically cannot be denied without denying that a certain principle of order applies to the particular row of color patches.

I believe we have said enough to show that we cannot in this context say all we want to say about different shades of blue and use abstract terms only as we use them in directive abstraction. With the latter alone we can direct attention to the specific blueness of each patch and to differences in shade among these various bluenesses, but we cannot in this way direct attention to the missing shade. What is missing is a representation or instance of a certain shade of blue and not simply a spot of ink;

a bit of cloth or anything else with the proper shade of blue placed on the paper would do just as well. The qualities by which we specify the missing shade are determined solely by qualities of the shades actually represented, and we cannot speak of the missing shade as something which is, but is not represented at a certain place, without speaking of the other shades as things which are, and which happen to be represented at the places in question. We can thus direct a person's attention to the missing shade only when he has already come to speak of the represented shades in this manner, and not merely as visible qualities of patches on paper. Only when, in other words, he has come to speak of them as abstract entities, as entities which have their own qualities irrespective of the concrete individuals which have them as qualities.

I shall call this use of abstract terms *hypostatic abstraction* (the phrase is borrowed from Peirce but I am not going to use it exactly as he did). The great importance of hypostatic abstraction, especially in mathematics, hardly needs emphasis. What does need emphasis, however, is the difference between directive and hypostatic abstraction. When these two are confused it is impossible to understand the presupposition of entities peculiar to the latter. We may speak of blueness, shades of blue, and blue colors without presupposing any entities besides blue individuals, for we may be doing nothing more than specifying ways of paying attention to such individuals. Yet a sure sign that we are doing more than this, that we are engaged in hypostatic abstraction and hence presupposing further entities, is our speaking of a certain color or shade as the one missing from a given representation of a color series. The existence of the missing color is deduced, not observed, and in deducing it we presuppose abstract entities in a serial order. To be sure, one may observe the gap in the row of colors representing the series, but merely observing the gap is not the same as recognizing the row of color patches as a representation of a definite color series. The point I want to emphasize is that the entities presupposed in hypostatic abstraction are always entities interrelated in a system of order, and that their existence is determined solely by the conditions which define the order.

Confusion on this point is inevitable if we speak of directive abstraction as also presupposing entities. Making sense out of the statement that the yellowness of a particular chair is an entity existing apart from the chair is quite different from making sense out of the statement that the color yellow is an entity which exists as a certain place in a series of colors. In a remark like, "Look at the number of books piled on his desk," the word "number" is clearly a term of directive abstraction and does not refer to an entity to be looked at in addition to the pile of books. Also, in replies like, "Yes, I count nineteen of them" or "I'm sure there are more than twenty," the number words signify results of looking at and attending to the pile of books and do not in this use presuppose abstract entities. But it is different with a reply like, "The number of books is equal to the number twenty." The phrase "the number twenty" like "the color yellow" refers to an abstract entity with a definite place in a serial order. Its use here indicates that the speaker is considering the pile of books as a representation or instance of such an order, just as we previously considered the row of color patches on a sheet of paper. If one counters with, "Then the number of books is necessarily greater than the number nineteen," the answer is that this is true only when the pile of books, like the row of color patches, is considered merely as a representation of a certain order. It is like the remark, "These patches are necessarily before those in the series of shades of blue."

Some expressions are used abstractly only in directive abstraction, and among these I would include names of materials. We might order different kinds of wood in degrees of increasing hardness, and we might speak in terms of hypostatic abstraction about degrees of hardness but never about kinds of wood. Having observed a gap in the series, we might say that there is a certain degree of hardness which is missing, but we would not say that there is a certain kind of wood which is missing. We would say rather that there is no known wood which possesses this degree of hardness. "Virtue," on the other hand, differs from "wood" and other names of materials in that it is sometimes used in hypostatic abstraction. Ethical philosophers have sometimes spoken of virtues as forming an ordered system, so that the

existence of a virtue is determined by a place in the system rather than by the fact that men actually possess it. In such a context, then, the presupposition of entities in the use of "virtue" is not the same as it is in the use of "wood," although it is the same when "virtue" is used in directive abstraction to direct attention to a man's habits or character, as when we say, "a virtuous man" or "a man of virtue."

This distinction between directive and hypostatic abstraction goes a long way, I believe, toward explaining the philosophic grammar which underlies our use of abstract terms and governs our presupposition of entities. But there are a number of points to be noted before we attempt to relate the distinction to various philosophic positions, and in particular to the view of abstract terms summarized at the beginning of this paper. It is now time to consider a widespread philosophic dogma associated with abstract terms.

VI

In considering this dogma, I am going to begin with terms which signify kinds of plants and animals and which are often called "names of plants and animals," since the dogma is usually stated with these terms as common examples. In brief, the dogma is that corresponding to every concrete general term, such as those signifying a kind of plant or animal, there are two abstract terms, one naming the attribute which any individual must possess in order to be of the kind in question and the other naming the class of individuals possessing this attribute. The attribute is then said to be the intention or connotation of the concrete term and the class to be its extension or denotation.

(i) According to a stock example, "man" and "human" are the concrete terms, "humanity" and "mankind" the corresponding abstract terms, naming, respectively, the attribute and the class. Let us note first that while neither "man" nor "human" is always used as the generic term for a certain kind of animal, this is the only use that is covered by the dogma. In a remark like, "He is more human than his brother but his brother is more of a man," both "man" and "human" have a derivative

or secondary use, a use which stems from and depends on their use as a generic term. They signify not that an individual is generically a man but that he possesses in some degree qualities which are generally thought to be characteristic of men, in the one case qualities that distinguish men from beasts and in the other case qualities that are associated with the masculine members of the species. The comparative forms "more human" and "more of a man" are appropriate only when "human" and "man" are given secondary and not generic use. If a scientist were to report, "We know it is almost as fatal to humans as to mice, since we know how to determine its effects on a man," it would be absurd to ask, "Is it more fatal to those who are more human?" or "Is it sure to be fatal to a person who is more of a mouse than a man?" These comparative forms show that the dogma applies only to generic terms in their primary use, since "man" and "human" in every use recognized by the dogma are supposed to connote the same humanity and denote the same individual men; there is no question about the humanity or the men varying in degree.

Yet, contrary to what the dogma would lead us to expect, the abstract form "humanity" generally provides an alternative expression only when "human" occurs in secondary use and not as a generic term. We might say, "He has more humanity than his brother," but the scientist would hardly begin his sentence in the form, "We know it is almost as fatal to individuals having humanity." As an alternative to a secondary use of "man" we have the abstract form "manhood," as in "His brother has more manhood." If the scientist were to say, "We know how to determine its effects on manhood," he would clearly mean something quite different from what he would mean by "its effects on a man." "Manhood" does not provide an alternative to "man" when the latter is used strictly as a generic term. There are also peculiarities in the grammar of "mankind" which one is not led to expect from the dogma, as, for instance, the fact that "mankind" can generally be replaced by "humanity." I shall comment on these peculiarities presently, but I want first to make some points specifically with reference to the use of "humanity" and "manhood."

When "man" and "human" are given secondary as opposed to generic use, the purpose is to compare and contrast particular individuals. Usually the individuals are men but there is no reason why they have to be. We might say that a machine or a nonhuman animal is human, is a man, has humanity or manhood, meaning that it has certain qualities characteristic of men, that in comparison with many other individuals it is more human, more manlike. This use of the abstract forms "humanity" and "manhood" is clearly directive and not hypostatic abstraction. We sometimes personify humanity and speak as if it were an entity by itself, but this is generally in poetic or metaphorical language and does not presuppose an entity in the way that our customary talk about the color yellow and the number twenty does. The grammar of "humanity" and "manhood" remains that of "yellowness" rather than "the color yellow." And I now want to urge the point that "humanity" and "manhood" have no proper use as abstract terms corresponding to "human" and "man" when the latter are used generically and not secondarily or, as we may also say, comparatively. In order to see this point we must consider briefly the use of expressions as generic terms.

Rather than characterizing an individual, a common noun such as "man" in its generic use identifies an individual. I do not mean identify as a witness identifies the criminal among a group of suspects but as a naturalist identifies an individual bird as a nuthatch. In the first case the identity claimed is absolute, simple, and unspecified; the witness claims that the suspect he picks is simply identical (numerically one and the same) with the criminal being sought. In the second case the identity claimed is relative and specified; the naturalist is not claiming that the bird he now sees and the nuthatch he saw yesterday are simply identical but only that they are identical generically, identical only when their identity is specified in a certain way in ornithological terms. Similarly, when we say that Peter and Paul are men, and we are using "man" generically; we mean that Peter and Paul are identical, though not simply but relatively—identical only when their identity is specified, for example, in a certain way in zoölogical terms. I shall call identity in this sense *relative identity*. An expression used as a generic term is one used to

signify the relative identity as opposed to the comparative similarity of individuals.

It is true that we detect relative identities by noticing the similarities of individuals and that we are inclined to regard a a generic term as characterizing the individuals it identifies. This circumstance tends to obscure the difference between statements of relative identity and statements of comparative similarity. Yet the difference is clearly indicated by the sort of statements we accept as incompatible with the original statement in the two cases. If in saying that Peter and Paul are men we mean only that they are identical generically, we would not accept statements like, "But Peter is more of a man" and "But Paul is more human," as incompatible with what we had stated. An incompatible statement would be one like, "But Paul is Peter's dog." Yet, if we are using "man" in a comparative or secondary sense and mean that Peter and Paul are comparatively similar as men, that one is just as much of a man or just as human as the other, then we would have to accept "But Peter is more of a man" or "But Paul is more human" as incompatible with what we had stated.[1]

Now if statements of relative identity do not assert comparative similarity, they cannot be paraphrased as asserting similarity in the possession of a quality. "Peter and Paul share alike in humanity" can serve as a paraphrase of "Peter and Paul are human" or "Peter and Paul are men" only when "human" and "man" are used in a secondary or comparative sense. It will not change matters if we say simply, "Peter and Paul have humanity," as this sentence is ambiguous. "Have humanity" is an expression with which comparative forms are always appropriate, and when the context does not make it clear that the expression can be understood as "share alike in humanity" it is always

[1] Of course, we might be using "man" in an ethical or legal context and mean that Peter and Paul are not to be distinguished as morally responsible individuals or as subjects before the law. But then the phrases "morally responsible individual" and "subject before the law" have uses which parallel the generic and secondary uses of "man" we have been discussing. Statements like "Peter is morally irresponsible" and "Paul is utterly lawless" are appropriate in comparing individuals and are not incompatible with simple assertions of ethical and legal identity.

appropriate to ask the speaker to specify the degrees. The expression "are men," on the other hand, when "man" is used generically is not ambiguous and a request to specify the degrees would be meaningless. The same is true for the singular forms, "Peter is a man," "Peter is human," "Peter has humanity," and "Peter has manhood." Questions of degree can always arise in connection with the last two cases, but they can arise in connection with the first two only when "man" and "human" are used in a secondary or comparative sense. But we have, then, no use for "humanity" and "manhood" as abstract terms corresponding to "human" and "man" when the latter are used generically.

The grammar of generic terms in taking comparative forms is thus that of names as opposed to what I shall call *adjectives*, that is, terms used to signify comparative similarity. If there is some doubt as to whether an individual is Peter, one might say, "But he's more Peter than Paul." However, if one is sure that the individual is Peter, it would be absurd to ask in what degree is he Peter rather than Paul. Similarly, if there is some doubt as to whether an individual is a plant or an animal, one might say, "But it's more plant than animal." He would not say this, however, if he were sure it was a plant. Of course, we may use names in stating comparisons and say, "Peter is more Paul than Peter when he acts that way" and "This plant is more animal than plant when it eats insects." But in these cases we are obviously using the names elliptically for the adjectival phrases "more like Paul than Peter" and "more animallike than plantlike." With adjectives, in contrast to names, comparative forms are usually appropriate even when one is sure the adjective applies. Exceptions to this occur when we agree to use adjectives with the precision we normally reserve for names of abstract entities. In saying that certain things are medium yellow, for example, we may agree to regard them simply as representations or instances of that color which occurs at a certain place in a color series. In this case, comparative forms are ruled inappropriate and every medium yellow thing is regarded as equally medium yellow. Only with this way of talking can we speak of exact measurement, of things having precisely the same color, weight, length, shape, and so on. The advantages of talking this way

are obvious, and we may do so without confusion in our philosophic grammar if we are careful to distinguish adjectives used in this absolute sense from generic terms and names of abstract entities.[2]

(ii) It should now not be difficult to see where the dogma in question goes wrong. The dogma regards all statements of relative identity as statements of comparative similarity, and thus treats generic terms as adjectives. While in some statements of the dogma, generic terms are called "common names," this expression is also applied to adjectives so that the confusion remains the same. Terms of directive abstraction like "humanity," "manhood," and "yellowness" supplement the use of adjectives but not that of generic terms, as the dogma claims. Finally, abstract terms which are class expressions do not always have a corresponding term of directive abstraction. This is true only when the concrete term is an adjective, as the class of yellow things can be called "the class of things having yellowness." A phrase like "the class of things which are instances of the color yellow" is something different. This phrase has a use only in a context where "yellow" is used in an absolute sense, where we want to speak of things as having precisely the same color rather than being more or less of the same color. On the other

[2] Confusion is perhaps most likely to arise from the use of "identity." We might say, "He identified the spot as medium yellow," and here the identity is absolute rather than generic or relative, if we take the object identified to be a particular abstract entity. Yet the sentence hardly means that every medium yellow spot is absolutely identical with every other; it means rather that the spots are identical only when their size, shape, and other factors are overlooked and their identity is specified only with reference to a certain series of colors. But then the adjective "medium yellow" would seem to identify objects relatively and hence to be a generic term. The catch lies in the word "spot." If we speak of identifying color spots, then "medium yellow" is a generic term; but we usually speak of identifying colors and of comparing spots with respect to their color, and in this case "medium yellow" as applied to a spot remains an adjective, even when it is used in an absolute sense. A similar situation might arise in talking about different metal disks used as weights in a balance. We might say, "He identified the smallest disk as half an ounce." Then we are using "half an ounce" as a generic term if we mean only to identify disk weights; but we usually identify weights by numerical values and compare metal disks with respect to their weight. In the latter case, "half an ounce" applied to a metal disk would of course function as an adjective.

hand, when the concrete term is generic we do not speak of a corresponding class in either of these ways. When we use "man" generically we may also use some such phrase as "the zoölogical species man" or, simply, "the human species." But the phrases, "the class of individuals having humanity" or "the class of individuals which are instances of humanity," would be inappropriate. "Mankind" has a special use generally associated with human values (as when we speak of what is proper to mankind, or approved by all mankind) and is appropriate when "man" or "human" is used secondarily or adjectivally rather than generically. It is thus that "humanity" in these contexts rather than "the zoölogical species man" is an alternative for "mankind." The expressions "the human race" and "the human species" are somewhat ambiguous and might be used in either context.

Expressions with the surface grammar of class expressions are not always abstract terms, let alone terms of hypostatic abstraction. "The human race" as used by geneticists names a concrete rather than an abstract entity, and when used as "mankind" or "humanity" it provides a way of talking about and calling attention to men collectively but it does not name an abstract entity. On the other hand, in expounding the logic of classes we may use expressions like "the class of men" and "the class of blue individuals" to refer to entities which we regard simply as ordered by logical relations. In this case, our language presupposes abstract entities. When we say, for example, that given any two classes there is a third which is their logical product, we are asserting the existence of an entity determined solely by a system of logical relations. We may use the expression "the class of blue men" to name such an entity, but we are not likely to use it as a way of calling attention to a group of men collectively. It is sometimes convenient in our talk about the logic of classes to distinguish between identical classes by saying that corresponding to each there is a different attribute. Along with names of classes we may thus use names of attributes, and corresponding to the latter it is convenient to have an expression which characterizes any individual possessing the attribute. A triad from ordinary language like "mankind," "humanity," and

"human" seems ready-made for illustrating the point, and there is certainly no harm in using the words for this purpose as long as we recognize that we are giving them a special use and are not analyzing the philosophic grammar underlying their ordinary use.

The above remarks suggest that the dogma in question represents an attempt to develop philosophic grammar from the language we use in talking about the logic of classes, and if there is anything to the suggestion it would seem that the view of abstract terms summarized at the beginning of this paper represents a similar attempt with the logic of classes supplemented by quantificational logic. In the latter we distinguish expressions occurring in positions accessible to quantifiable variables from other expressions which apply to entities. Expressions of the first kind are the only ones which technically are names; expressions of the second kind are technically predicates. We thus obtain two forms of discourse in which we can say what is equivalent to saying that a given individual belongs to a given class or possesses a given attribute. We may use a sentence in which a predicate is applied to a named individual, or we may name a class or an attribute and use a sentence in which the named individual is said to belong to the named class or to possess the named attribute. Since existence is asserted only by the quantifier and since only names occur in positions accessible to variables of quantification, it is only in the second form of discourse that we can deduce from our original sentence one asserting the existence of classes or attributes as well as individuals. There is thus a distinction, not made in the logic of classes alone, between two equivalent forms of discourse, only one of which presupposes classes or attributes.

VII

In conclusion, I would like to summarize briefly how what has been said about abstract terms modifies the view of them we obtain from quantificational logic. To begin with, the neat distinction between names and predicates cannot be sustained outside the special language we use in developing quantificational

logic. The criterion of purporting to apply to one and only one object will not do as a way of determining names in philosophic grammar. It not only overlooks the grammatical similarity between generic terms and proper names, but it also makes terms of directive and hypostatic abstraction equally names.

The old expression "common name" when limited to generic terms serves to emphasize that the latter are distinct from adjectives as well as both similar to and different from proper names. While we tend to regard a generic term as characterizing the individuals it identifies, it is more accurate to say that it only identifies them. Of course, if one knows the use of "man" and is told that Peter is a man, he can proceed without further information to characterize Peter in a way that will distinguish him from other animals. But also if one knows the use of "Peter" and is told that a certain individual is Peter, he can proceed without further information to characterize that individual in a way that will distinguish him from other men. The reason for saying that in neither case does the name characterize the individual is the way the names take comparative forms and not the way knowledge of their use enables one to characterize the individuals they apply to. The fact that we speak of defining "man" but not "Peter" is a result of the difference between a common and a proper name, and not of the difference between an adjective and a name.

Terms of directive abstraction are difficult to classify and there is no reason to expect that we can always draw neat distinctions in philosophic grammar. The mere failure to take plural forms in the way that concrete general terms usually take these forms is not enough to make the expression a name, and terms like "humanity" and "yellowness" (which take comparative forms) seem better regarded as special forms of adjectives than as names. "The quality of being yellow" certainly has the look of a name, but in its use as an expansion of "yellowness" it serves to direct attention to a condition of a concrete object rather than to name a separate entity. Similar remarks hold for "piety" and "virtue" when these terms are used in directive abstraction. Names of materials are especially troublesome. When we say "The chemist identified the contents of the vial as arsenic," we seem clearly

to mean relative or generic identity. Yet a question of degree in this case is always appropriate, and as we have noted, names of materials often function as terms of directive abstraction. Perhaps the best suggestion is to put names of materials in a class by themselves.

Finally, the presupposition of entities in hypostatic abstraction is not simply a question of what expressions are used in positions accessible to variables of quantification, although admittedly within the language we use in developing quantificational logic this is the way we distinguish hypostatic abstraction from other forms of discourse. The point is that the quantifier always has the same function, regardless of the variables it ranges over; it does not permit us to distinguish senses of "existence." While such distinction may be irrelevant to quantificational logic, it is indispensable in understanding the philosophic grammar of hypostatic abstraction. I have made the suggestion that at least one peculiarity of "existence" as used in hypostatic abstraction is that we say a certain entity exists solely because of what is determined by the relations which define a system of order. We thus not only deduce the existence of an entity but we dismiss as irrelevant the question of independent support from observation, and this is something we never do in asserting the existence of a concrete individual. This suggestion needs to be explored at length before we can accept it, but in any event I believe the exploration would be philosophically fruitful. And in the process we might become clearer as to what we mean by a concrete as opposed to an abstract entity.

The philosophic position usually called "Platonic realism" is misrepresented if we say it is nothing more than the acceptance of hypostatic abstraction. Such abstraction is commonplace in our ordinary discourse and there is no question of getting along without it. But there is certainly a question as to whether it should be taken as the model for all discourse. It is one thing to say that in talking simply about a certain shade of blue we are talking about an entity which exists solely as a place in an ordered series of colors, and quite another thing to say that in talking simply about a nuthatch we are talking about an entity which exists solely as a place in an ordered series of animals.

Yet only the latter seems characteristic of Platonic realism. Philosophic nominalism, on the other hand, is hardly to be represented as a position which simply dispenses with hypostatic abstraction and keeps everything else intact. It seems better to characterize it as a position which tries to make directive abstraction do the whole job. However, I do not believe the next step in our consideration of abstract terms should be to attempt precise characterizations of "realism" and "nominalism." There are many more specific points to be attended to first, and in the process of seeing to these we may discover that the issues of realism and nominalism do not represent important alternatives.

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HOBBES: THE TAYLOR THESIS

I

IN 1938, A. E. Taylor advanced the bold thesis that Hobbes's ethical theory is logically independent of the egoistic psychology and is a strict deontology.

> There are really two distinct questions before Hobbes, the question why I *ought* to behave as a good citizen, and the question what inducement can be given me to do so if my knowledge of the obligation to do so is not in itself sufficiently effective. According to his repeated declarations, it is a certain fact of psychology that I shall violate the law and break the peace if I believe that I stand to gain by doing so. . . . But the Hobbian answer to the other question, why I ought, or am obliged, to be a good citizen is quite different; it is quite explicitly that I have, expressly or tacitly, pledged my word to be one, and to violate my word, to refuse to "perform my covenant as made," is *iniquity*, *malum in se*. Hobbes's ethical doctrine proper, disengaged from an egoistic psychology with which it has no logically necessary connection, is a very strict deontology, curiously suggestive, though with interesting differences, of some of the characteristic theses of Kant.[1]

In the two full decades that have now elapsed since the publication of Taylor's paper, most historians of philosophy and political theory have ignored, as misleading and too curious, the similarities between Hobbes and Kant to which Taylor called attention.[2] But few have ignored Taylor's principal contention that Hobbes's ethical theory, considered on its merits and as distinct from the psychology, is in some sense a deontology and not an explication of moral and political obligation in terms of prudence and self-interest. Indeed, the arguments and evidence adduced by Taylor in support of his thesis are so compelling that most of the recent work on Hobbes has been done on the presumption that the thesis is true. With the exception of Thomas

[1] A. E. Taylor, "The Ethical Doctrine of Hobbes," *Philosophy*, XIII (1938), 407-408.

[2] See, for example, Howard Warrender, *The Political Philosophy of Hobbes* (Oxford, 1957), pp. 57-58, 87-93, 222-233, 335-337.

Nagel's recent paper,[3] which is a sensitive and careful defense of the traditional view of Hobbes, controversies about the interpretation of Hobbes's theory have been for the most part between scholars who agree with each other in accepting Taylor's thesis and who disagree only as to what in detail Hobbes's nonprudential theory of obligation is.

In *The Political Philosophy of Hobbes*, one of the fullest commentaries ever written on a major political philosopher, Warrender accepts the Taylor thesis and, in several passages, states the thesis in ways that clearly echo Taylor.

> Hobbes says so much about self-preservation that it is easily regarded as being central in his theory of obligation. This is so far from being the case that it is not a part of that theory as such, but an empirical postulate employed in its application. A denial of Hobbes's psychology, therefore, merely poses a new problem of application, but leaves his theory of obligation, in the proper sense, unaffected.[4]

> Thus [according to Hobbes], the reason why I *can* do my duty is that I am able (with adequate deliberation, etc.) to see it as a means to my preservation; but the reason why I *ought* to do my duty is that God commands it.[5]

Warrender contends, that is, that there is in Hobbes an ethical theory *proper*, a theory of obligation *in the proper sense*, and that this is logically independent of the empirical statements which comprise Hobbes's psychology and his view of the world.

Again, though never stated explicitly, the Taylor thesis is implicit in Oakeshott's discussion of obligation in his Introduction to the widely used Blackwell edition of *Leviathan*. Oakeshott distinguishes four different kinds of obligation which, for the sake of convenience, he calls *physical*, *rational*, *moral*, and *political*.[6] Despite Oakeshott's claim that all four kinds of obligation are to be found in Hobbes, even on his own showing only *rational* and *moral* obligation are of any particular theoretical interest. *Physical* obligations obtain in cases of external, physical restraints

[3] Thomas Nagel, "Hobbes's Concept of Obligation," *The Philosophical Review*, LXVIII (1959), 68 ff.

[4] Warrender, *op. cit.*, p. 93.

[5] *Ibid.*, p. 213.

[6] Michael Oakeshott, Introduction to *Leviathan* (Oxford, 1946), pp. lviii-lxi.

upon action;[7] whereas *political* obligations are mixed, "consisting of physical, rational, and moral obligation, combined to serve one end, but never assimilated to one another."[8] It is Oakeshott's distinction between *rational* and *moral* obligation that is central to and crucial for his interpretation, for political obligation consists of these two plus the physical restraints imposed by an enforcing power. But as Oakeshott treats it, *rational* obligation is a psychological phenomenon. It obtains in cases of psychological restraints upon action, where "the impediment is internal, a combination of rational perception and fear."[9] A psychological restraint upon action is, however, quite distinct from moral obligation which implies a restriction on natural right and is to be explained in terms of the covenant effecting this restriction. "The answer to the question, Why am I morally bound to obey the will of this Sovereign? is, Because I have authorized this Sovereign, 'avouched' his actions, and am 'bound by my own act.' "[10] But moral obligation, in this *proper sense* implying a restriction upon natural right, is "not based upon self-interest."[11] Self-interest is a merely rational obligation, an internal and psychological phenomenon which supplies no principle for the authorization of the sovereign. In this manner Oakeshott disengages Hobbes's ethical theory proper from the egoistic psychology.

There are at least three excellent reasons why Taylor, Oakeshott, Warrender, and others[12] insist upon making so radical a

[7] *Ibid.*, p. lix.
[8] *Ibid.*, p. lxi.
[9] *Ibid.*, p. lix.
[10] *Ibid.*, p. lx.
[11] *Ibid.*

[12] See, for example: (1) S. P. Lamprecht, "Hobbes and Hobbism," *American Political Science Review*, XXXIV (1940), 31 ff.; and Introduction to *De cive* (New York, 1949), pp. xv ff. (2) J. M. Brown, "A Note on Professor Oakeshott's Introduction to the *Leviathan*," *Political Studies*, I (1953), 53 ff. And (3) Dorothea Krook, "Mr. Brown's Note Annotated," *Political Studies*, I (1953), 216 ff. Lamprecht's distinction between "Hobbes" and "Hobbism" is, in principle, a distinction between an interpretation of Hobbes based on the Taylor thesis and the traditional interpretations. Although J. M. Brown sharply disagrees with Oakeshott, Brown's own interpretation of Hobbes appears to be based on the Taylor thesis. Dorothea Krook, however, seems inconsistent: on the one hand, she defends Oakeshott against Brown on points

distinction between Hobbes's ethical theory and his psychology. First, there are the arguments and evidence presented by Taylor. These consist primarily of passages in which Hobbes discusses obligation in language that cannot be explained on the traditional, egoistic interpretation: the familiar passages in which Hobbes asserts that men are obliged *in foro interno* to keep covenants even in a state of nature, treats obligation in terms of covenanting and submitting, and lists the duties of sovereigns to their subjects. There are passages of this sort in all three of Hobbes's major works in political philosophy; and for any such passage in one of the works, there is usually a parallel passage in at least one of the other two. This being the case, one must assume either that Hobbes is systematically inconsistent or that the traditional interpretation is somehow mistaken and in need of substantial revision. The first alternative is implausible and profitless.

Secondly, on the traditional view, Hobbes's ethical theory must be false or untenable if psychological egoism is false. But Hobbes's psychological egoism, interpreted as a descriptive account of motivation, now seems to us incredibly crude and plainly false. Thus if we are to take Hobbes seriously and read him sympathetically with a view to determining whether or not his ethical theory is tenable, we must regard the traditional interpretation with deep suspicion. We must assume that his ethical theory could turn out to be logically independent of his psychology.

And thirdly, even if Hobbes himself did not in fact distinguish clearly between questions in moral philosophy and questions in empirical psychology, we now distinguish between these questions and must determine whether Hobbes could in principle have done so without damage to his theory.

For reasons such as these, Taylor, Oakeshott, Warrender, and others have read and written about Hobbes from a point of view radically different from that of the tradition. To these men members of the present generation of political philosophers are indebted for the freshness of mind with which they now can read Hobbes and for their newly deepened understanding of him.

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of interpretation which are plausible only on the Taylor thesis; on the other hand, she makes statements about Hobbes and about weaknesses in Oakeshott's interpretation that imply the denial of the Taylor thesis.

Nonetheless, the Taylor thesis is false: Hobbes does not in fact hold, and cannot in principle admit, that the statements comprising his psychology have no logical bearing on the statements comprising his ethical theory. I shall now proceed to establish that the thesis is false and that the interpretations of Hobbes, based on the thesis, completely misrepresent him.

II

Although any one of several considerations can be employed to prove the Taylor thesis false, I shall state only two.

First, there is Hobbes's own repeated and consistent testimony about what he is doing in the political treatises. According to Hobbes, the principal doctrines of a political theory have practical implications adverse to the interests of men and, for this reason, tend to remain unsettled and controversial.[13] To overcome this difficulty and to gain some measure of acceptance for his theory, the political philosopher must establish his principal doctrines by a rigorous argument from premises whose truth cannot be denied. In each one of the three political treatises, Hobbes claims to overcome the difficulty in just this way.

> To reduce this doctrine [of justice and policy in general] to the rules and infallibility of reason, there is no way, but first to put such principles down for a foundation, as passion not mistrusting, may not seek to displace; and afterward to build thereon the truth of cases in the law of nature (which hitherto have been built in the air) by degrees, till the whole be inexpugnable.[14]

> Having therefore thus arrived at two maxims of human nature, . . . I seem from them to have demonstrated by a most evident connection, in this little work of mine, first the absolute necessity of leagues and contracts, and thence the rudiments both of moral and of civil prudence.[15]

[13] Hobbes, *The Elements of Law*, ed. by Tönnies (Cambridge, 1928), The Epistle Dedicatory, p. xvii. All subsequent references to the *Elements of Law* are to this edition.

[14] *Ibid.*, p. xvii.

[15] Hobbes, *De cive* (ed. by Lamprecht), The Epistle Dedicatory, p. 5. All subsequent references to *De cive* are to this edition.

> And as to the whole doctrine, I see not yet, but the principles of it are true and proper; and the ratiocination solid. For I ground the civil right of sovereigns, and both the duty and liberty of subjects, upon the known natural inclinations of mankind, and upon the articles of the law of nature; of which no man, that pretends but reason enough to govern his private family, ought to be ignorant.[16]

In this manner Hobbes, speaking for himself, directly contradicts the Taylor thesis. He explicitly denies, and on his own view of political theory cannot even in principle admit, that his ethical theory is independent of his psychology. As an account of Hobbes in terms of what Hobbes himself thinks and is trying to accomplish, the Taylor thesis is therefore false.

By directly contradicting Hobbes those who adopt the Taylor thesis expound Hobbes on the presumption that he fails to make out the logical relationships required by his theory. On the Taylor thesis, one does not establish Hobbes's failure by a direct examination of his arguments. Taylor does not even acknowledge that his thesis implies Hobbes's failure and takes no cognizance whatsoever of Hobbes's own testimony. Oakeshott thinks that Hobbes may have failed on some standards of logical rigor and theoretical adequacy, but insists that the standards on which he fails are our own, modern standards which cannot legitimately be imposed on seventeenth-century thinkers.[17] Warrender takes brief notice of Hobbes's testimony only by way of rejecting it on the merely general grounds that Hobbes's thought is much less rigorous than is commonly supposed.[18] Presuming failure, one proceeds, as Warrender puts it, "to piece together Hobbes's argument in so far as it may legitimately be done."[19]

But clearly, the standards of theoretical adequacy and logical rigor are Hobbes's own: they are self-imposed by a political philosopher who, whatever his failures, argues with extraordinary power and singleness of purpose. To disregard these standards, to interpret Hobbes as if any such standards were irrelevant, and

[16] Hobbes, *Leviathan* (ed. by Oakeshott), A Review and Conclusion, pp. 465-466. All subsequent references are to this edition.

[17] Oakeshott, *op. cit.*, p. lxiii.

[18] Warrender, *op. cit.*, p. 2.

[19] *Ibid.*

to piece together out of Hobbes a body of doctrine logically independent of his theory, is to emasculate him. It is to present, as Hobbes's theory, a set of doctrines which he does not articulate and support by arguments and which, it is admitted, cannot logically be supported by his arguments. For what Hobbes demands of political theory is logical support for moral and political doctrines which would otherwise remain unestablished and controversial; and what on the Taylor thesis is presented as his theory is a set of doctrines which, lacking the required support, cannot constitute the theory.

Viewed in this light, the Taylor thesis must either deny that Hobbes has a theory or fail to present a significant alternative to the traditional interpretation. Both Oakeshott and Warrender seem at times to accept the first alternative. They both appear to suppose that Hobbes's thought is too lacking in clarity, precision, and rigor to state a systematic theory of the sort Hobbes himself thinks he is presenting.[20] But on this supposition interpretations based on the Taylor thesis are absurd. For if Hobbes's thought is so loose as to justify ignoring what he presents as his theory, it is absurd for a commentator to expound as Hobbes's real theory a set of doctrines which satisfy strict tests of clarity and consistency. The only acceptable alternative is the second; viz., that the Taylor thesis emphasizes a side of Hobbes which has generally been ignored but fails to present a significant alternative to the traditional interpretation. What characterizes the traditional view is that it takes Hobbes seriously as a philosopher arguing step by step to establish the details of his position. On the traditional view the question as to whether Hobbes succeeds or fails is never begged nor settled on merely general grounds; it is answered, always with regard to specific steps in his arguments, by a detailed analysis of the relevant passages. But there is nothing inconceivable about a political philosopher, Hobbes or any other, importing into his discussions of obligation a number of moral doctrines which either cannot be derived from or cannot be made consistent with logically primary theses of his position. If Hobbes does this at any crucial point in his

[20] Oakeshott, *op. cit.*, p. lxviii; Warrender, *op. cit.*, pp. 1-2.

argument, then he blunders, and the argument is defective at just this point. The traditional view neither denies that Hobbes may blunder by importing such moral doctrines into his theory nor that these moral doctrines, once isolated, may turn out to be self-consistent and illuminating of Hobbes's own moral convictions.[21] What the traditional view denies is (1) the propriety of treating a self-consistent set of moral doctrines, illicitly imported into a theory, as constituting the theory itself; and (2) the propriety of assuming without proof that a set of moral doctrines has been illicitly imported into a theory. Only out of confusion can one be led to affirm what the traditional view denies. The value of work on the Taylor thesis would be substantially increased rather than diminished if it were presented not as a new interpretation alternative to the traditional one but as an elucidation of a side of Hobbes which the tradition, with its strong philosophical bias, has tended to ignore. Unencumbered by the necessity to treat doctrine as theory, the Taylor thesis could then be employed to illuminate Hobbes, as a man whose own moral and political convictions were not at every point compatible with the logical implications of his argument. In any case, the traditional view is in no way weakened or superseded, as Nagel has shown, by confusing Hobbes with Hobbism and presenting his moral convictions as his ethical theory.[22]

The second consideration which proves the Taylor thesis false

[21] A moral or political philosopher who pursues his argument relentlessly to conclusions that are incompatible with his own deep moral convictions may nonetheless at times express these convictions in his philosophical writings. When this occurs, there will be a body of moral doctrine stating the convictions as distinct from and inconsistent with a body of philosophical doctrine stating the conclusions of the argument. The question is whether or not there is a systematic inconsistency in Hobbes's writings and, if there is, whether it is explicable in this way. I neither beg nor answer this question here. The point is that, whatever the correct answer may be, the traditional interpretation can accommodate it.

[22] "Hobbism" is simply a name for Hobbes's theory as it was understood by Hobbes's contemporaries and by the subsequent tradition of British moral and political philosophy. To substitute "Hobbes" for "Hobbism" is to introduce in a question-begging way a new theory never systematically expounded by Hobbes himself. As Dorothea Krook points out (*op. cit.*, pp. 223-224), this may conceal from us those elements in Hobbes's theory which outraged his contemporaries and which repel many modern readers.

is that the moral doctrine, pieced together out of Hobbes and expounded sympathetically as a self-consistent theory, is in fact inconsistent and philosophically untenable.

Those who adopt the Taylor thesis must present on Hobbes's behalf a theory in which the concept of obligation is moral, as distinct from prudential or legal, and in which this moral concept is explicated without recourse to psychological considerations. But this task is impossible of fulfillment. For in Hobbes the notion of covenant is employed as an indispensable logical link between political obligation on the one hand and psychological considerations on the other. It is employed to connect the obligation to obey the law with those human desires and aversions which give every citizen a stake in the institution of government. The duties of citizenship, that is, presuppose an obligation made out in terms of covenant, and covenanting presupposes interests at stake. On the Taylor thesis this chain of presuppositions must be broken at a point where the moral considerations will lie together completely isolated from considerations of interest explicated in psychological terms. But the moral notions in Hobbes are so connected, logically, with psychological ones that they cannot be isolated and still make sense. Thus the concept of obligation actually isolated by Taylor and the others turns out to be legal rather than moral. But Taylor, Oakeshott, and Warrender wish to expound for Hobbes a distinctively moral theory of obligation. They therefore introduce moral considerations to which neither they nor Hobbes, on this view of him, are entitled; and the moral doctrine attributed to Hobbes is inconsistent.

In Oakeshott this inconsistency shows up as a flat self-contradiction. His first explanation of the obligation to obey the sovereign is stated in terms of covenanting and is moral: the reason why I am obliged to obey is that "I have authorized this Sovereign, 'avouched' his actions, and am 'bound by my own act.' "[23] But in the second sentence following, this moral explanation is flatly contradicted and restated in terms of positive law: the covenant itself is not "morally obligatory and, not being

[23] Oakeshott, *op. cit.*, p. lx.

a law (the will of the Sovereign), it does not itself make any conduct morally obligatory." Oakeshott then goes on to say that "this and any other covenant may become obligatory if and when the sovereign authority commands its observation."[24] Thus, Oakeshott's account of obligation is, in the end, legal rather than moral, though he tries, at the cost of self-contradiction, to make it moral rather than legal.

Precisely the same difficulty is present in Taylor's original paper. Taylor begins by asserting that, according to Hobbes, a man is obliged to be a good citizen because he is party to a covenant and has pledged his word to be one.[25] But Taylor then proceeds to argue that all obligation, including the obligation to honor covenants made, is derived by Hobbes from the laws of nature conceived as the commands of God.[26] Again, the contradiction: I am obliged because I have covenanted; but as there would be no obligation to keep covenants unless some authority, God or civil sovereign, commands it, covenants of themselves do not oblige.

Warrender expounds Hobbes in such a way as to conceal the difficulty. Elaborating an argument in Taylor, Warrender holds that the laws of nature must be interpreted in one or the other of two logically distinct ways. Either they must be interpreted in terms of the psychological theory, as prudential rules concerning what men, similarly constituted and similarly situated, will do after deliberation; or they must be interpreted in terms of the legal theory, as laws properly so called and the commands of God. But there is no moral obligation to obey rules stating how men will act after deliberation and consideration of all relevant facts. Because Hobbes believes that there are moral obligations in a state of nature to seek peace and keep covenants, and because these obligations are formulated in terms of natural law, natural law in Hobbes must be interpreted as laws properly so called, the commands of God. Unless they are so interpreted, Warrender insists, Hobbes has no distinctively moral theory.[27]

[24] *Ibid.*
[25] Taylor, *op. cit.*, p. 408.
[26] *Ibid.*, pp. 419-420.
[27] Warrender, *op. cit.*, pp. 212-213. 220, 232-233.

Thus Warrender always states the reason why men are obliged in terms of God's commands. He avoids the contradiction, explicit in Taylor and Oakeshott, by never giving Hobbes's account of obligation solely in terms of a covenant or pledge.

But this conceals the difficulty at the very same cost of attributing an absurdity to Hobbes. For Warrender concludes his account of the covenant as follows:

> Not all of the obligations of the citizen derive from covenant and his own consent; there exist obligations logically prior to those contracted which are imposed by natural laws. The first of these laws commands us to seek peace and from this is derived, among other principles, the rule itself which requires men to honor valid covenants. Logically, therefore, the prior instrument of obligation is natural law which does not fall under covenant but guarantees the obligatory character of what has been covenanted, and the ground of obligation is revealed when the duty to observe natural law is explained.[28]

The absurdity in these passages is the clear implication that there could in principle be valid covenants without, however, any obligation to fulfill them. For natural law, interpreted as the command of God, does not guarantee performance. It only guarantees, what the notion of covenant implies, the obligation to perform.

The same kind of absurdity comes out in a different but even more serious way in Warrender's treatment of the difference between atheists and theists with respect to obligation.[29] The atheist understands the laws of nature as maxims of prudence. He does not understand and in fact denies that they are the commands of God and therefore laws in the strict sense. For this reason the atheist has no obligations. He stands to God as an enemy in a state of war. The theist, on the other hand, is obliged, just because he acknowledges the existence of God, God's sovereignty in a natural kingdom, and the laws of nature as God's commands. But on this account natural law has no intrinsic moral content; otherwise the atheist would be obliged to obey them. Moreover, law in the strict sense, the command of a

[28] *Ibid.*, p. 248.
[29] *Ibid.*, pp. 315-317.

sovereign one has covenanted to obey, implies no obligation; otherwise, again, the atheist would be obliged and it would not be necessary to provide a guarantee of the obligatory character of covenants. What follows from this, however, is that no one, not even the theist, is obliged. For to say that natural law is the command of God is simply to say that a set of rules, lacking in moral content, is law in the strict sense. If an atheist cannot incur an obligation to obey civil law, simply by acknowledging the status of the civil sovereign, then neither I nor anybody can incur an obligation to obey natural law simply by acknowledging the status of God as sovereign in a kingdom of nature. What Warrender attributes to Hobbes as a theory of obligation is a position on which no one can be obliged at all. Absurd as this consequence is, it is not surprising if one begins by supposing that considerations of private interest are morally irrelevant, that one can covenant without incurring obligations, and that acknowledging the status of the civil sovereign implies no duties.

Because of these absurdities which must be attributed to Hobbes on the Taylor thesis, the thesis fails. The whole point of adopting the thesis is to expound for Hobbes a moral theory that is free of major inconsistencies and contradictions. The contention is that Hobbes's theory can be made self-consistent and philosophically defensible if it is interpreted as logically independent of the psychology. But this contention is false. Either the old and familiar difficulties in Hobbes are reappearing in a new guise or altogether new difficulties, based on mistakes which he himself does not make, are being created for him. Regardless of which alternative is correct, the Taylor thesis fails.

In fact, however, the second alternative is correct; the Taylor thesis creates altogether new difficulties for Hobbes, and these difficulties are based on mistakes which he did not make.

III

In all of the political treatises the principal question before Hobbes is, Why are men obliged to submit unconditionally to the civil state? Hobbes is fully convinced that men are obliged to submit unconditionally and is fully aware that this conviction

is extremely controversial. His task, therefore, is to establish this obligation by rigorous argument from premises which are uncontroversial. In the political treatises Hobbes's interest in any other questions is ancillary or peripheral. It is, therefore, a mistake to suppose, with Taylor and Warrender, that Hobbes is addressing himself to two logically distinct questions and that these questions are, Why ought I to do my duty? and, How is it psychologically possible for me to do it? The first of these questions implies that men already know what their duties are but do not know why they ought to do them. This leads directly to the absurdity of requiring some guarantee for the obligatory character of valid covenants and conceals from us the necessity of establishing the extremely controversial thesis concerning unconditional submission. The second question, concerning how it is psychologically possible for me or anyone to do his duty, is of only peripheral interest. For if the obligation of unconditional submission can be established, the sovereign is justified in employing the sanctions necessary to secure obedience. Finally, to assume that Hobbes is asking these two questions implies, what is false, that he wants to make a radical distinction between strictly moral and strictly prudential questions. Hobbes makes no such distinction. What he claims to establish in *De cive* are "the rudiments both of moral and of civil prudence."[30] Indeed, it would be a fatal defect in his argument if the obligation of unconditional submission were based wholly on nonprudential considerations.

The principal question before Hobbes is not prima facie a philosophical question at all. It is a question in moral and civil prudence about how to establish the particular obligation of unconditional submission to the civil state. One temptingly easy answer to this question is not open to Hobbes. Although he may be entitled as a consequence of his enquiry to define obligation in terms of law, he cannot initially and directly appeal to any such definition. For this would beg the question and fail to convince anyone. A person who seriously doubts whether it is ever obligatory to keep covenants shows, by his doubts, that he does

[30] *De cive*, The Epistle Dedicatory, p. 5.

not understand what covenanting is. But a person who doubts the obligation of unconditional submission to law does not, in any analogous way, show that he fails to understand what law or obligation is. What such a person doubts and denies is that legal duties, which he may acknowledge, take precedence over all others, including prudential obligations and the dictates of private conscience. Hobbes's argument must be designed to resolve the doubts of a man who already understands obligation in the sense implied by valid covenants and who also understands law in the sense implied by theft, trespass, attainder, and the like.

Now, there are two distinct grounds for doubting Hobbes's contention concerning unconditional submission. There are moral and prudential grounds. These consist of widely accepted rules or principles which in certain conceivable circumstances justify acts of civil disobedience. There are, in addition, philosophical grounds for doubt. These consist of certain logical considerations which show that the notion of obligation is conditional, and of the puzzle as to how, this being so, there can be an obligation to submit unconditionally.

The beauty of Hobbes's theory is that both of these two grounds for doubt are disposed of in one direct argument. That obligation is logically a conditional notion is not only admitted; the conditions are made out in considerable detail. Our use of ought, Hobbes insists, is meaningful only if we presuppose or take for granted both that men can do their duty and that duties on the whole are directly or indirectly productive of what men themselves regard as their own good.[31] But from this it follows that there will be an obligation to submit unconditionally to law only if, in terms of what men regard as their own good, the stake of each man in the preservation of civil government is of supreme magnitude. Hobbes undertakes to establish that the stake is of this sort. The argument establishing this meets the moral, as distinct from the philosophical, objection to his principal contention. It is given mainly in the psychology, and employs psychological concepts like desire and aversion. Whether or not it is completely successful, it is remarkably strict.

[31] *Leviathan*, ch. 14, pp. 86-87, 90-91.

In bare outline the argument is as follows:

1. Men strive ceaselessly to obtain certain objects and to avoid others; but different men, and even one man at different times, strive for different things.[32]

2. The objects men strive to obtain they call good; those they strive to avoid they call evil.[33]

   2.1. Each man of necessity desires his own good.[34]

   2.2. What one man counts as good another may count as evil.

3. Power is but a man's means of obtaining goods and avoiding evils.[35]

   3.1. Men ceaselessly desire power.

   3.2. The power men actually seek is relative both to the aims of a particular man and to each man's private assessment of the means he must accumulate in order to assure future success.

4. Men as individuals are substantially equal with respect to natural or original, as distinct from acquired, power.[36]

   4.1. Success in obtaining what one wants does not depend upon the superiority of any one natural power, like physical prowess.

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[32] *Leviathan*, ch. 6, pp. 31-32, and ch. 11, p. 63. *De cive*, 14.17, p. 166. This is the principal empirical premise of Hobbes's argument.

[33] *Leviathan*, ch. 6, p. 32. *De cive*, 14.17, p. 166. *The Elements of Law*, II. 7.3, p. 29. This stipulates uses for "good" and "evil." What a given man calls "good" in this sense may not, however, turn out to be a good for that man. Mistakes are possible, but any mistake must be made out in terms of the desires and strivings of the man who commits it.

[34] *Leviathan*, ch. 14, p. 86. *Elements of Law*, I. 14.12, p. 14; and I. 17.14, pp. 17-18. This is a tautology based upon the stipulated uses for "good" and "evil." The tautology which allows for the possibility of mistakes is: "each man chooses his greatest apparent good."

[35] *Leviathan*, ch. 6, pp. 37-39; ch. 10, p. 56; ch. 11, p. 64. *Elements of Law*, II. 8.3-4, pp. 33-34; II. 7.7, p. 30.

[36] *Leviathan*, ch. 13, p. 80. *De cive*, 1.3, p. 25. *Elements of Law*, I. 14.2, p. 70. Although Hobbes argues as if this were an empirical premise, the second such premise in the argument as I restate it, I suspect that this contention about the equality of men may be based upon the tautology, "no natural power is irresistible."

4.2. Men are substantially equal in their inability as individuals to defend themselves and their acquisitions against ravage.

5. The absence of civil government is—properly called—a state of nature.[37]

5.1. The state of nature implies the absence of institutional restraints upon the acquisition of power and, except for self-help, no protection of persons and goods.

5.2. The state of nature is a state of war, of each man against every other, wherein no man can reasonably expect to obtain goods and be secure in the enjoyment of them.

6. Any rules stating the conditions necessary if any man is to obtain goods and specifying the acts required equally of all men if these conditions are to be fulfilled are laws of nature.[38]

6.1. Civil peace is such a condition; and the rules specifying the acts and endeavors required equally of all men if peace is to be secured are laws of nature.

6.2. "He that foreseeth the whole way to his preservation . . . must also call it good, and the contrary evil. And this is that good and evil, which not every man in passion calleth so, but all men by reason. And therefore, the fulfilling of all these laws is good in reason; and the breaking of them evil."[39]

7. The primary condition of peace is that each man stand out

[37] Although it is a mistake to interpret Hobbes's account of the state of nature as a description of some prehistoric stage in the development of human institutions, it is also a mistake to interpret it as having no bearing upon history and therefore as wholly unhistorical. The state of nature is a practically possible condition of man which can be closely approximated in the course of history and which has evil consequences avoidable only by submission to law. With only a few refinements Hobbes's account of the state of nature can be used to provide reasons why the people of all nations are obliged as individuals to submit to a sovereign international government.

[38] *Leviathan*, ch. 14, pp. 84-85, 104-105. *De cive*, 2.1-2, pp. 31-33; 3.19, p. 52. *Elements of Law*, 1.15.1, pp. 74-75.

[39] *Elements of Law*, 1.17.14, p. 94. *Leviathan*, ch. 15, p. 104-105. *De cive*, 3.31, pp. 57-58.

of the way of every other man, leaving all men generally secure in their pursuit and enjoyment of goods.[40]

7.1. In order for men to be willing to stand out of each other's way, and in order for it to be reasonable for them to do so, there must be an effective form of protection other than self-help.

7.2. Other than self-help, the only protection against ravage is institutional, some form of civil law.

7.3. It is therefore good in reason and a law of nature that each man submit unconditionally to law.

8. The moral practice by which men most commonly obtain goods for themselves while simultaneously securing the good of another is covenanting.

8.1. Even in a state of nature it would be good in reason for a man, taken captive by an enemy, to obtain his life and liberty by covenanting to pay a ransom; and any such covenant is valid.[41]

8.2. The stakes at issue in the alternatives between civil peace and war are comparable in magnitude to those at issue in the alternatives between life at the cost of ransom and death.

8.3. The obligation to submit unconditionally to law may be regarded, by those who have never acknowledged it or by those who seriously question it, as incurred by a kind of covenant in which life and liberty are purchased at the cost of submission.

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[40] *Leviathan*, ch. 14, p. 85. *De cive*, 2.4, p. 33. *Elements of Law*, II. 15.3, p. 76.

[41] *Leviathan*, ch. 14, p. 91; ch. 15, p. 96. Hobbes states explicitly that even in a state of nature some covenants are valid and that men are obliged to fulfill promises by reason of having promised (see *De cive*, 14.2, p. 157). So far as I can determine, Hobbes never doubts the general validity of covenants and thus never requires any guarantee of their obligatory character. What Hobbes doubts is (1) that valid covenants in a state of nature will be honored, and (2) that in a state of nature men will covenant with each other except in extreme cases like those in which a man purchases life and liberty at the cost of ransom. In the state of nature the social utility, not the validity of covenanting, is impaired. That there is a moral practice of covenanting is thus an implicit premise in Hobbes's argument.

IV

This is Hobbes's argument.[42] It is elegant, compelling, and remarkable for its economy in the use of straightforward empirical statements. The main weight of the argument is borne by logical statements, stipulations and tautologies based on stipulations. It must be so borne in order to satisfy Hobbes's own criteria of theoretical adequacy. In his account of what he calls science and we would call theory, he insists that science is not knowledge of experienced matters of fact, but "knowledge of all the consequences of names appertaining to the subject in hand."[43] Whereas empirical statements do not justify the assertion or presumption of logically necessary connections, theoretical statements establish, assert, or imply such connections. Statements like, "Men necessarily desire their own good" or "Men necessarily desire self-preservation," must therefore be interpreted as logical statements. Contrary to one of the crucial contentions of those who adopt the Taylor view, Hobbes's psychology does not consist primarily of empirical statements, and his doctrines concerning egoism and self-preservation cannot be interpreted as empirical postulates which may be denied without damage to his theory.

Because Hobbes's egoism is not an empirical postulate, the laws of nature in Hobbes's theory cannot be related to his egoism as prudential maxims are related to particular desires and aversions. There is thus no justification for the view, advanced by Taylor and the others, that the laws of nature must be interpreted in one or the other of two logically distinct ways, either as prudential maxims which imply no obligation or as the commands of God which are obligatory.

Consider the prudential maxim, *Beware of little expenses: a small leak will sink a great ship.* This prudential maxim has great scope. Most people, it may be presumed, have an aversion to poverty,

[42] I do not claim that everything which Hobbes says is consistent with this restatement of the argument or that all of the passages cited in support of the Taylor thesis are consistent with it. What I claim is that this restatement of the argument is consistent with all of Hobbes's statements about method and with most of the passages in which he argues and expounds his political theory, whereas on both of these counts the Taylor thesis fails.

[43] *Leviathan*, ch. 5, p. 29.

and there is a connection between poverty and the cumulative effect of many small, unnoticed expenditures. Still one can imagine circumstances under which the maxim would lose its point. If one lives in a state where poverty, as mere lack of private capital, has no evil consequences for life and health, an aversion to poverty could not be presumed and the maxim would have at best so limited an application as to be of little or no use. Or one can imagine an economic order in which the cumulative effect of small expenditures does not impoverish or in which there are in fact no small expenses. In such cases the maxim has no application and is pointless.

On the Taylor thesis the laws of nature as prudential maxims are to be interpreted on this model. It is supposed that the laws of nature specify the courses of action required to satisfy some particular desire common to all or most people, a particular desire for self-preservation or a particular aversion to death. But then, of course, one can easily imagine men who would prefer to die rather than forego certain goods and suffer certain evils. To these men Hobbes's laws of nature would be either pointless or acceptable only in some form which does not imply unconditional submission. Even if all or most men were like this, however, it still might be the case that all or most men have in common some other particular desire immensely stronger than all other desires, and with respect to this strongest desire a new law of nature with different implications for political practice might be formulated. Considering the laws of nature in this way, Taylor and the others deny their moral relevance and their importance for ethical theory. But this denial commits a threefold error. It commits a moral error: if there are any laws of nature in this sense, they have direct implications for our moral practices and our obligations. It commits a philosophical error: even if in fact there are no such laws of nature, it is of great philosophical interest to consider the differences which such laws would make to our notions of good, ought, and so forth. Finally, it commits an error of historical interpretation: the laws of nature in Hobbes are not prudential maxims in this sense.

In Hobbes, the laws of nature are rules specifying what each and every man must do if the conditions for successfully employing

any means whatsoever are to be maintained. Even if in fact there happens to be some one desire common to all men, the laws of nature cannot be interpreted with exclusive reference to it. They can be interpreted only in terms of Hobbes's statements that men strive ceaselessly to obtain certain objects and that whatever men strive to obtain they call "good." The tautology, "every man necessarily desires his own good," is based upon the statement stipulating the use for "good," and the point of "his own" is to admit explicitly that what one man counts as good another may count as evil. Because this is a tautology, it cannot be denied on the empirical grounds that some men may actually strive to obtain for others what they want or that some men actually fear certain things more than their own death. Hobbes in fact admits that some men fear the humiliation of defeat more than death in combat.[44] Whatever a man desires is a good for him, in the primary sense of good. A man's assertion that something is good can be denied only by reference to that man's own desires or to the conditions for satisfying them. Peace is a good whether or not men in fact desire it, because peace is the condition for securing to each man whatever in fact he does desire. Thus, the only men for whom the laws of nature are pointless are those who either live in complete isolation from other men or desire no objects attainable by human foresight and effort. No natural class of creatures for which the laws of nature are in principle pointless can properly be called "men."

Although I myself think that Hobbes is correct in holding that there are laws of nature in this sense, the point is that, whether or not he is correct in holding this, he does hold it and employs the laws of nature to make out the obligation to submit to the state. But if there are thought to be laws of nature in this sense, then there is, just as Hobbes claims, an obligation with respect to them and this obligation does not require a command of God to guarantee it. Only out of sheer moral perversity could one acknowledge such laws of nature and deny that they oblige. Hobbes's argument based upon the laws of nature may, of course, be defective. It may be possible, as I think it is, to acknowledge

[44] *Elements of Law*, 1.9.6, p. 30.

the laws of nature and deny an obligation of unconditional submission to the state. But by denying this particular obligation in the unconditional form in which Hobbes tries to make it out, one does not deny that the laws of nature oblige in some way and in some form which can be made out. If these laws did not oblige, independently of being commanded by God, they would not be laws of nature. God, the creator and designer of nature, could not conceivably command disobedience to these laws or by command void the obligation with respect to them. In God's prophetic kingdom, Hobbes admits, God may by command give natural law the status of revealed laws. If we as believers conceive of God as doing this, then for us natural law is law in the strict sense. This is possible but not necessary.

In sum: Hobbes's psychology is indispensable because in it he establishes the sense of good in terms of which he argues that the stake of each man in the institution of government is of supreme magnitude. What is completely dispensable in Hobbes are prudential maxims and the commands of God. The Taylor thesis grossly misrepresents Hobbes because it dispenses with the indispensable and makes what is dispensable the very heart of the theory.

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WITTGENSTEIN'S PHILOSOPHY OF MATHEMATICS

FROM time to time Wittgenstein recorded in separate notebooks thoughts that occurred to him about the philosophy of mathematics. His recently published *Bemerkungen über die Grundlagen der Mathematik*[1] consists of extracts made by the editors from five of these. Neither it nor any of these notebooks was intended by its author as a book. That it cannot be considered, and ought not to be criticized, as such is therefore unsurprising, though disappointing. Many of the thoughts are expressed in a manner which the author recognized as inaccurate or obscure; some passages contradict others; some are quite inconclusive; some raise objections to ideas which Wittgenstein held or had held which are not themselves stated clearly in the volume; other passages again, particularly those on consistency and on Gödel's theorem, are of poor quality or contain definite errors. This being so, the book has to be treated as what it is—a selection from the jottings of a great philosopher. As Frege said of his unpublished writings, they are not all gold but there is gold in them. One of the tasks of the reader is therefore to extract the gold.

I encounter frequently in conversation the impression that this is typical of Wittgenstein's work in general; I have often heard the *Investigations* characterized as evasive and inconclusive. This seems to me a travesty of the truth; the book expresses with great clarity many forceful, profound, and quite definite ideas—though it is true that a hasty reader may sometimes be bewildered by the complexity of some of the thoughts. The contrast with the present volume is marked, and is due entirely to the different origins of the two books.

In the philosophy of mathematics, Platonism stands opposed to various degrees of constructivism. According to Platonism,

[1] *Bemerkungen über die Grundlagen der Mathematik*. By Ludwig Wittgenstein. Edited by G. H. von Wright, R. Rhees, and G. E. M. Anscombe. With English translation (*Remarks on the Foundations of Mathematics*) by G. E. M. Anscombe, on facing pages. Oxford, Basil Blackwell, 1956. Pp. xix, 196, each version.

mathematical objects are there and stand in certain relations to one another, independently of us, and what we do is to discover these objects and their relations to one another. The constructivist usually opposes to this the picture of our making, constructing, the mathematical entities as we go along. For the Platonist, the meaning of a mathematical statement is to be explained in terms of its truth-conditions; for each statement, there is something in mathematical reality in virtue of which it is either true or false. An example of the explanation of meaning in terms of truth and falsity is the truth-table explanation of the sentential connectives. For the constructivist, the general form of an explanation of meaning must be in terms of the conditions under which we regard ourselves as justified in asserting a statement, that is, the circumstances in which we are in possession of a proof. For instance, a statement made up of two statements joined by a connective is to be explained by explaining a claim to have proved the complex statement in terms of what a claim to have proved the constituent statements consists in; thus a claim to have proved ⌜A or B⌝ will be a claim to have a method leading either to a proof of A or to a proof of B. What in practice this will lead to will depend upon the degree of constructivism adopted; for example, if we confine ourselves to decidable statements, then the truth-tables will receive an acceptable interpretation and the whole classical logic will be applicable; if, on the other hand, we allow with the intuitionists a much wider range of mathematical statements to be considered as intelligible, then the law of excluded middle and many other classically valid laws will cease to hold generally. But in either case it is the notion of proof and not the notions of truth and falsity which is for the constructivist central to the account of the meaning of mathematical statements.

We may regard Platonism and the various varieties of constructivism not as rivals but merely as means of demarcating different areas of mathematics with respect not to subject matter but to methods of proof. In this case there are only the essentially mathematical problems of formulating clearly the different conceptions and investigating in detail the mathematical consequences of each. If, on the other hand, one regards the different

schools as rivals, there remains the philosophical problem of deciding which of the various accounts is correct. Wittgenstein's book is intended as a contribution to the latter task only. It seems natural to suppose that the philosophical task and the mathematical go hand in hand, for the precise formulation of a conception is not irrelevant to deciding on its correctness, and unexpected consequences of adopting it may lead one to revise one's opinion as to its value. Wittgenstein will have none of this: for him philosophy and mathematics have nothing to say to one another; no mathematical discovery can have any bearing on the philosophy of mathematics.[2] It would seem that he is theoretically committed also to the converse, that no philosophical opinion could, or at least ought to, affect the procedure of the mathematician. This comes out to some extent in his discussion of the law of excluded middle in mathematics. Against one who insisted that either the sequence "77777" occurs in the development of π or it does not, he employs arguments similar to those of the intuitionists; and yet it appears that he is not wishing to question the validity in a mathematical proof of, for example, argument by cases, but only to reprove someone who in the course of philosophical reflection wishes to insist on the law of excluded middle.[3] Yet this is not to be taken too seriously, for Wittgenstein would always be able to claim that, while he had not shown that certain mathematical procedures were *wrong*, still he had shown them not to have the interest we were inclined to attach to them. Certainly in his discussion of Cantor he displays no timidity about "interfering with the mathematicians."[4] I think that there is no ground for Wittgenstein's segregation of philosophy from mathematics but that this springs only from a general tendency of his to regard discourse as split up into a number of distinct islands with no communication between them (statements of natural science, of philosophy, of mathematics, of religion).

As Frege showed, the nominalist objection to Platonism—that talk about "abstract entities" is unintelligible—is ill-taken; if we

[2] Cf. V, 13, 19; IV, 52; also *Investigations*, II, xiv; I, 124.
[3] IV, 10.
[4] I, App. II.

believe in the objectivity of mathematics, then there is no objection to our thinking in terms of mathematical objects, nor to the picture of them as already there waiting to be discovered that goes with it. Nor is formalism a real alternative. The formalist insists that the content of a mathematical theorem is simply that *if* there is any domain for which the axioms hold good, then the theorem will also hold good for that domain; and he will add that so long as we do not know the axioms to be categorical, a statement of the theory need not be either true or false. But he will not reject the classical logic, since he will agree that in any particular domain for which the axioms hold, the statement will be either true or false; and furthermore, he will allow that any given statement either does or does not follow from the axioms. Since the statement that there exists a proof of a given statement from given axioms is in exactly the same position as, say, an existence-statement in number theory for which we have neither proof nor disproof, the formalist has gained no advantage; he has merely switched from one kind of mathematical object—numbers—to another—formal proofs.

Wittgenstein adopts a version (as we shall see, an extreme version) of constructivism; for him it is of the essence of a mathematical statement that it is asserted as the conclusion of a *proof*, whereas I suppose that for a Platonist a being who had *direct* apprehension of mathematical truth, not mediated by inferences, would not be a complete absurdity. There are many different lines of thought converging upon Wittgenstein's constructivism; I shall deal first with his conception of logical necessity.

A great many philosophers nowadays subscribe to some form of conventionalist account of logical necessity, and it is perhaps difficult to realize what a liberation was effected by this theory. The philosophical problem of necessity is twofold: what is its source, and how do we recognize it? God can ordain that something shall hold good of the actual world; but how can even God ordain that something is to hold good in all possible worlds? We know what it is to set about finding out if something *is* true; but what account can we give of the process of discovering whether it *must* be true? According to conventionalism, all

necessity is imposed by us not on reality, but upon our language; a statement is necessary by virtue of our having chosen not to count anything as falsifying it. Our recognition of logical necessity thus becomes a particular case of our knowledge of our own intentions.

The conventionalism that is so widespread is, however, a modified conventionalism. On this view, although all necessity derives from linguistic conventions that we have adopted, the derivation is not always direct. Some necessary statements are straightforwardly registers of conventions we have laid down; others are more or less remote *consequences* of conventions. Thus "Nothing can at the same time be green and blue all over" is a direct register of a convention, since there is nothing in the ostensive training we give in the use of color-words which shows that we are not to call something on the borderline between green and blue "both green and blue." "Nothing can be both green and red," on the other hand, is necessary in consequence of the meanings of "green" and "red" as shown in the ostensive training. We did not need to adopt a special convention excluding the expression "both green and red" from our language, since the use by someone of this expression would already show that he had not learned what he was supposed to have learned from the ostensive training.

When applied to mathematics, this modified conventionalism results in the sort of account of mathematical truth with which we are so familiar from logical positivist writings. The axioms of a mathematical theory are necessary in virtue of their being direct registers of certain conventions we have adopted about the use of the terms of the theory; it is the job of the mathematician to discover the more or less remote consequences of our having adopted these conventions, which consequences are epitomized in the theorems. If it is inquired what is the status of the logical principles in accordance with which we pass from axioms to theorems, the reply is that to subscribe to these principles is again the expression of the adoption of linguistic conventions, in this case conventions about the use of "if," "all," and so forth. This account is entirely superficial and throws away all the advantages of conventionalism, since it leaves unexplained the status of the

assertion that certain conventions have certain consequences. It appears that if we adopt the conventions registered by the axioms, together with those registered by the principles of inference, then we *must* adhere to the way of talking embodied in the theorem; and *this* necessity must be one imposed upon us, one that we meet with. It cannot itself express the adoption of a convention; the account leaves no room for any further such convention.

Wittgenstein goes in for a full-blooded conventionalism; for him the logical necessity of any statement is always the *direct* expression of a linguistic convention. That a given statement is necessary consists always in our having expressly decided to treat that very statement as unassailable; it cannot rest on our having adopted certain other conventions which are found to involve our treating it so. This account is applied alike to deep theorems and to elementary computations. To give an example of the latter, the criterion which we adopt in the first place for saying that there are n things of a certain kind is to be explained by describing the procedure of counting. But when we find that there are five boys and seven girls in a room, we say that there are twelve children altogether, without counting them all together. The fact that we are justified in doing this is not, as it were, implicit in the procedure of counting itself; rather, we have chosen to adopt a *new* criterion for saying that there are twelve children, different from the criterion of counting up all the children together. It would seem that, if we have genuinely distinct criteria for the same statement, they may clash. But the necessity of "$5 + 7 = 12$" consists just in this, that we do not count anything as a clash; if we count the children all together and get eleven, we say, "We must have miscounted."

This account is very difficult to accept, since it appears that the mathematical proof drives us along willy-nilly until we arrive at the theorem. (Of course, we learned "$5 + 7 = 12$" by rote; but we could produce an argument to prove it if the need arose.) But here Wittgenstein brings in the considerations about rules presented in the *Investigations* and elsewhere. A proof proceeds according to certain logical principles or rules of inference. We are inclined to suppose that once we have accepted the axioms

from which the proof starts, we have, as it were, no further active part to play; when the proof is shown us, we are mere passive spectators. But in order to follow the proof, we have to recognize various transitions as applications of the general rules of inference. Now even if these rules had been explicitly formulated at the start, and we had given our assent to them, our doing so would not in itself constitute recognition of each transition as a correct application of the rules. Once we have the proof, we shall indeed say that anyone who does not accept it either cannot really have understood or cannot really have accepted the rules of inference; but it does not have to be the case that there was anything in what he said or did before he rejected the proof which revealed such a misunderstanding or rejection of the rules of inference. Hence at each step we are free to choose to accept or reject the proof; there is nothing in our formulation of the axioms and of the rules of inference, and nothing in our minds when we accepted these before the proof was given, which of itself shows whether we shall accept the proof or not; and hence there is nothing which *forces* us to accept the proof. If we accept the proof, we confer necessity on the theorem proved; we "put it in the archives" and will count nothing as telling against it. In doing this we are making a new decision, and not merely making explicit a decision we had already made implicitly.

A natural reaction to this is to say that it is true enough when we have not formulated our principles of inference, or have formulated them only in an imprecise form, but that it does not apply at all when we have achieved a strict formalization. Wittgenstein's hostility to mathematical logic is great; he says that it has completely distorted the thinking of philosophers.[5] Because this remark as it stands is so plainly silly, it is difficult to get a clear view of the matter. Consider a favorite example of Wittgenstein's: you train someone to obey orders of the form "Add n" with examples taken from fairly small numbers, then give him the order "Add one" and find that he adds two for numbers from 100 to 199, three for numbers from 200 to 299, and so forth. Wittgenstein says that there need have been nothing

[5] IV, 48.

either in what you said to him during the training or in what "went on in your mind" then which of itself showed that this was not what you intended. This is certainly true, and shows something important about the concept of intention (it is a very striking case of what Wittgenstein means when he says in the *Investigations* that if God had looked into my mind, he would not have been able to see there whom I meant). But suppose the training was not given only by example, but made use also of an explicit formulation of the rule for forming from an Arabic numeral its successor. A machine can follow this rule; whence does a human being gain a freedom of choice in this matter which the machine does not possess?

It would of course be possible to argue that someone might appear to understand a rule of inference in a formal system —a substitution rule, say—and yet later reject a correct application of it; but it remains that we can see *in* the precise wording of the rule that that application was warranted. It might be replied that this is to take for granted the ordinary understanding of the words or symbols in terms of which the rule is framed; an explanation of these words or symbols would be something like Wittgenstein's idea of a rule for interpreting the rule. It is undoubtedly true and important that, while in using a word or symbol we are in some sense following a rule, this rule cannot in its turn be formulated in such a way as to leave no latitude in its interpretation, or if it can, the rules for using the words in terms of which this rule is formulated cannot in their turn be so formulated. But such considerations seem to belong to the theory of meaning in general, rather than having any particular relevance to the philosophy of mathematics. Rather, it seems that to someone who suggests that Wittgenstein's point about the scope left in deciding on the correctness of an application of a rule of inference is to be countered by concentrating on rules of inference in formal systems we ought to reply by referring to what Wittgenstein calls the "motley" of mathematics.[6] He wishes, like the intuitionists, to insist that we cannot draw a line in advance round the possible forms of argument that may

[6] II, 46, 48.

be used in mathematical proofs. Furthermore, it might be pointed out that a formal system does not *replace* the intuitive proofs as, frequently, a precise concept replaces a vague intuitive one; the formal system remains, as it were, answerable to the intuitive conception, and remains of interest only so long as it does not reveal undesirable features which the intuitive idea does not possess. An example would be Gödel's theorem, which shows that provability in a formal system cannot do duty as a substitute for the intuitive idea of arithmetical truth.

Suppose we are considering a statement of some mathematical theory. To avoid complications, assume that the theory is complete, that is, that it can be completely formalized, but that we are not thinking of any particular formal system. Then a Platonist will say that there exists either a proof or a disproof of the statement; the fact that the statement is true, if it is true, consists in the existence of such a proof even though we have not yet discovered it. Now if there exists a proof, let us suppose that there is somewhere an actual document, as yet unseen by human eyes, on which is written what purports to be a proof of the statement. Then Wittgenstein will reply that all the same there does not yet exist a proof, since when we discover the document it is still up to us to decide whether or not we wish to count it as a proof. It is evident that, if this is correct, then all motive for saying with the Platonist that there either *is* or *is not* a proof, that the statement must be either true or false, and so forth, has gone. What is not clear to me is that rejecting the Platonist's conception involves adopting this line about proofs; it seems to me that a man might hold that, once the proof was discovered, we had no choice but to follow it, without allowing the correctness of saying, before the proof was discovered, that either there is a proof or there is not. I will return to this later.

Wittgenstein's conception is extremely hard to swallow, even though it is not clear what one wishes to oppose to it. The proof is supposed to have the effect of persuading us, inducing us, to count such-and-such a form of words as unassailably true, or to exclude such-and-such a form of words from our language. It seems quite unclear how the proof accomplishes this remarkable feat. Another difficulty is the scarcity of examples. We naturally

think that, face to face with a proof, we have no alternative but to accept the proof if we are to remain faithful to the understanding we already had of the expressions contained in it. For Wittgenstein, accepting the theorem is adopting a new rule of language, and hence our concepts cannot remain unchanged at the end of the proof. But we could have rejected the proof without doing any more violence to our concepts than is done by accepting it; in rejecting it we could have remained equally faithful to the concepts with which we started out. It seems extraordinarily difficult to take this idea seriously when we think of some particular actual proof. It may of course be said that this is because we have already accepted the proof and thereby subjected our concepts to the modification which acceptance of the proof involved; but the difficulty of believing Wittgenstein's account of the matter while reading the proof of some theorem with which one was not previously familiar is just as great. We want to say that we do not know what it would be like for someone who, by ordinary criteria, already understood the concepts employed, to reject this proof. Of course we are familiar with someone's simply not following a proof, but we are also familiar with the remedy, namely to interpolate simpler steps between each line of the proof. The examples given in Wittgenstein's book are—amazingly for him—thin and unconvincing. I think that this is a fairly sure sign that there is something wrong with Wittgenstein's account.

Consider the case of an elementary computation, for example "$5 + 7 = 12$." There might be people who counted as we do but did not have the concept of addition. If such a person had found out by counting that there were five boys and seven girls in a classroom, and were then asked how many children were present, he would proceed to count all the children together to discover the answer. Thus he would be quite prepared to say that on one occasion there were five boys, seven girls, and twelve children altogether, but on another occasion five boys, seven girls, and thirteen children altogether. Now if we came across such a person, we should know what kind of arguments to bring to show him that in such circumstances he must have miscounted on one occasion, and that whenever there are five boys and

be used in mathematical proofs. Furthermore, it might be pointed out that a formal system does not *replace* the intuitive proofs as, frequently, a precise concept replaces a vague intuitive one; the formal system remains, as it were, answerable to the intuitive conception, and remains of interest only so long as it does not reveal undesirable features which the intuitive idea does not possess. An example would be Gödel's theorem, which shows that provability in a formal system cannot do duty as a substitute for the intuitive idea of arithmetical truth.

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seven girls there are twelve children. If he accepts these arguments it will be quite true that he will have adopted a new criterion for saying that there are twelve children present, and again a new criterion for saying, "I must have miscounted." Before, he would say, "I miscounted," only when he noticed that he had, for example, counted one of the children twice over; now he will say, "I miscounted," when he has not observed anything of this kind, simply on the ground that he got the result that there were five boys, seven girls, and thirteen children. But we wish to say that even before we met this person and taught him the principles of addition, it would have been true that if he had counted five boys, seven girls, and thirteen children, he would have been wrong even according to the criteria he himself then acknowledged. That is, he must have made a mistake in counting; and if he made a mistake, then there must have been something that he did which, if he had noticed it, he himself would then have allowed as showing that he had miscounted.

If we say that if he counted five boys, seven girls, and thirteen children, then there must have been something which, if he had noticed it, he would have regarded as a criterion for having miscounted, then the effect of introducing him to the concept of addition is not to be simply described as persuading him to adopt a new criterion for having miscounted; rather, he has been induced to recognize getting additively discordant results as a *symptom* of the presence of something he already accepted as a criterion for having miscounted. That is, learning about addition leads him to say, "I miscounted," in circumstances where he would not before have said it; but if, before he had learned, he had said, "I miscounted," in those circumstances, he would have been right by the criteria he then possessed. Hence the necessity for his having miscounted when he gets additively discordant results does not, as it were, get its whole being from his now recognizing such results as a criterion for having miscounted.

If on the other hand we say that it is possible to count five boys, seven girls, and thirteen children without there being anything other than the fact of getting these results such that,

if we had noticed it, we should have regarded it as a ground for saying that we had miscounted, then it appears to follow that one can make a mistake in counting (according to the criteria *we* recognize for having miscounted) without having made any particular mistake; that is, one cannot say that if one has miscounted, then either one counted this boy twice, or one counted that girl twice, or . . . . But this is absurd: one cannot make some mistake without there having been some particular mistake which one has made. It might be replied that we can choose to say that if one has miscounted, then either . . ., and that that is in fact what we do choose to say. But if a disjunction is true, then at least one of its limbs must be true; and if a statement is true, there must be something such that if we knew of it, we would regard it as a criterion for the truth of the statement. Yet the assumption from which we started is that someone counts five boys, seven girls, and thirteen children (and hence says that he must have miscounted) and that there is nevertheless nothing apart from his having got these results which (if he knew of it) he would regard as showing that he had miscounted; and hence there can be nothing which (if he knew of it) would show the truth of any one of the disjuncts of the form "He counted that boy twice," and so forth. One might put it by saying that if a disjunction is true, God must know which of the disjuncts is true; hence it cannot be right to count something as a criterion for the truth of the disjunction whose presence does not guarantee the existence of something which would show the truth of some one particular disjunct. For example, it would be wrong to regard ⌜Either if it had been the case that P, it would have been the case that Q, or if it had been the case that P, it would have been the case that not Q⌝ as a logical law, since it is perfectly possible to suppose that however much we knew about the kind of fact which we should regard as bearing on the truth of the disjunct counterfactuals, we should still know nothing which we should count as a reason for accepting either the one or the other.

It is certainly part of the meaning of the word "true" that if a statement is true, there must be something in virtue of which it is true. "There is something in virtue of which it is true"

means: there is something such that if we knew of it we should regard it as a criterion (or at least as a ground) for asserting the statement. The essence of realism is this: for any statement which has a definite sense, there must be something in virtue of which either it or its negation is true. (Realism about the realm of mathematics is what we call Platonism.) Intuitionists do not at all deny the first thesis; for them one is justified in asserting a disjunction only when one has a method for arriving at something which would justify the assertion of some one particular limb of the disjunction. Rather, they deny the second thesis: there is no reason for supposing in general that, just because a statement has a quite definite use, there must be something in virtue of which either it is true or it is false. One must beware of saying that logical truths are an exception, that there is nothing in virtue of which they are true; on the contrary, for the realist we are justified in asserting ⌜P or not P⌝ because there must be something in virtue of which either P or ⌜Not P⌝ is true, and hence in any case there must be something in virtue of which ⌜P or not P⌝ is true.

Now there seems here to be one of the big differences between Wittgenstein and the intuitionists. He appears to hold that it is up to us to decide to regard any statement we happen to pick on as holding necessarily, if we choose to do so.[7] The idea behind this appears to be that, by laying down that something is to be regarded as holding necessarily, we thereby in part determine the sense of the words it contains; since we have the right to attach what sense we choose to the words we employ, we have the right to lay down as necessary any statement we choose to regard as such. Against this one would like to say that the senses of the words in the statement may have already been fully determined, so that there is no room for any further determination. Thus, if one takes a classical (realist) view, the general form of explanation of the sense of a statement consists in the stipulation of its truth-conditions (this is the view taken by Wittgenstein in the *Tractatus* and also the view of Frege). Thus the sense of the sentential operators is to be explained by means

[7] Cf. V, 23, last par. on p. 179.

of truth-tables; it is by reference to the truth-tables that one justifies taking certain forms as logically true.

Since the intuitionist rejects the conception according to which there must be for every statement something in virtue of which either it is true or it is false (and does not regard it as possible to remedy the situation by the introduction of further truth-values), for him the fundamental form of an explanation of a statement's meaning consists in stating the criteria we recognize as justifying the assertion of the statement (in mathematics, this is in general the possession of a proof). We thus specify the sense of the sentential operators, of "or," for example, by explaining the criteria for asserting the complex statement in terms of the criteria for asserting the constituents; hence, roughly speaking, we are justified in asserting ⌜P or Q⌝ only when we are justified either in asserting P or in asserting Q. A logical law holds in virtue of these explanations; by reference to them we see that we shall *always* be justified in asserting a statement of this form.

Wittgenstein's quite different idea, that one has the right simply to *lay down* that the assertion of a statement of a given form is to be regarded as always justified, without regard to the use that has already been given to the words contained in the statement, seems to me mistaken. If Wittgenstein were right, it appears to me that communication would be in constant danger of simply breaking down. The decision to count a particular form of statement as logically true does not affect only the sense of statements of that form; the senses of all sorts of other statements will be infected, and in a way that we shall be unable to give a direct account of, without reference to our taking the form of statement in question as logically true. Thus it will become impossible to give an account of the sense of any statement without giving an account of the sense of every statement, and since it is of the essence of language that we understand *new* statements, this means that it will be impossible to give an account of the use of our language at all. To give an example: suppose someone were to choose to regard as a logical law the counterfactual disjunction I mentioned above. We try to object to his claim that this is logically valid by observing that either he must admit that a disjunction may be true when neither limb

is true, or that a counterfactual may be true when there is nothing in virtue of which it is true, that is, nothing such that if we knew of it we should regard it as a ground for asserting the counterfactual. But he may respond by denying that these consequences follow; rather, he adduces it as a consequence of the validity of the law that there must be something such that if we knew of it we should count it as a ground either for asserting ⌜If it had been the case that P, then it would have been the case that Q⌝ or for asserting ⌜If it had been the case that P, then it would have been the case that not Q.⌝ For example, he will say that there must be something in which either the bravery or the cowardice of a man consisted, even if that man had never encountered danger and hence had never had an opportunity to display either courage or cowardice. If we hold that he is entitled to regard anything as a logical law which he chooses so to regard, then we cannot deny him the right to draw this conclusion. The conclusion follows from the disjunction of counterfactuals which he elected to regard as logically true in the first place, together with statements we should all regard as logically true; and in any case, he must have the right to regard the conclusion itself as logically true if he so chooses. He will thus conclude that either a man must reveal in his behavior how he would behave in all possible circumstances, or else that there is inside him a sort of spiritual mechanism determining how he behaves in each situation.

Now we know from the rest of Wittgenstein's philosophy how repugnant such a conclusion would be to him; but what right would he have, on his own account of the matter, to object to this man's reaching this conclusion? It is all very well to say, "Say what you like once you know what the facts are"; but how are we to be sure that we can tell anyone what the facts are if it may be that the form of words we use to tell him the facts has for him a different sense as a result of his having adopted some logical law which we do not accept? It might be said that once we discover this difference in the understanding of a certain form of words, we must select another form of words which he does understand as we do and which expresses what we wanted to say; but how are we to know that there is a form of words which

does the trick? If we ask him how he understands a certain statement, and he gives the same explanation of it that we should give, this is no guarantee that he in fact understands it as we do; for the mere fact that he recognizes certain forms as logically true which we do not recognize means that he may be able to construct arguments leading to the given statement as a conclusion and with premises that we accept, although we should not accept the argument; that is, he will regard himself as entitled to assert the statement in circumstances in which we should not regard ourselves as entitled to assert it. (An analogy, *not* strictly parallel, is this: we might imagine a classicist and an intuitionist giving explanations of the meaning of the existential quantifier which sounded exactly the same. Yet for all that the classicist will make existential assertions in cases in which the intuitionist will not, since he has been able to arrive at them by means of arguments which the intuitionist will not accept.) Now in the case we are imagining, it is essential to suppose that our man is not capable of giving any general kind of explanation of the words he uses such that we can, from this explanation, derive directly the meaning he attaches to any sentence composed of these words. For if he could give such an explanation, we could see from the explanation why the logical law which he accepts but we do not *is* necessary if the words in it are understood as he understands them. We should thus have a justification for taking statements of that form to be logical laws parallel to the justification of the laws of classical logic in terms of an explanation of meaning by reference to truth-conditions and the justification of intuitionist logic in terms of the explanation by reference to assertibility-conditions. But the whole point of the example was that this was a case of simply laying down a certain form of statement as logically true without the requirement of a justification of this kind.

This attitude of Wittgenstein's to logical necessity may in part explain his ambivalence about the law of excluded middle in mathematics. If a philosopher insists on the law of excluded middle, this is probably the expression of a realist (Platonist) conception of mathematics which Wittgenstein rejects: he insists that ⌜P or not P⌝ is true because he thinks that the general form

of explanation of meaning is in terms of truth-conditions, and that for any mathematical statement possessing a definite sense there must be something in virtue of which either it is true or it is false. On the other hand, if a mathematician wishes to use a form of argument depending upon the law of excluded middle (for example, ⌜If P, then Q⌝; ⌜If not P, then Q⌝; therefore, Q), Wittgenstein will not object, since the mathematician has the right to regard the form of words ⌜P or not P⌝ as holding necessarily if he chooses to do so.

To return to the example of the people who counted but did not have addition, it seems likely that someone who accepted Wittgenstein's viewpoint would wish to reject the alternative: either when one of these people counted five boys, seven girls, and thirteen children there must have been something which, if he had noticed it, would have been for him evidence of his having miscounted, or else he could have done so when there was nothing which would have shown him he had miscounted. He would reject it on the ground that it is unclear whether the alternative is being posed in *our* language or in the language of the people in question. *We* say that he must have miscounted, and hence that he must either have counted this boy twice, or . . ., and hence that there was something which if he had noticed it would have shown him that he had miscounted, and we say this just on the ground that his figures do not add up. But he would have no reason for saying it, and would assert that he had probably counted correctly. Now we must not ask whether what we say or what he says is *true*, as if we could stand outside both languages; we just *say* this, that is, we count his having got discordant results as a criterion for saying it, and he does not. Against this I wish, for the reasons I have stated, to set the conventional view that in deciding to regard a form of words as necessary, or to count such-and-such as a criterion for making a statement of a certain kind, we have a responsibility to the sense we have already given to the words of which the statement is composed.

It is easy to see from this why Wittgenstein is so obsessed in this book with an empiricist philosophy of mathematics. He does not wish to accept the empiricist account, but it has a strong

allure for him; again and again he comes back to the question, "What is the difference between a calculation and an experiment?". The fact is that even if we decide to *say* that we must have made a mistake in counting when we count five boys, seven girls, and thirteen children, our mere decision to treat this result as a criterion for having made a mistake cannot of itself make it probable that in such circumstances we shall be able to find a mistake; that is, if Wittgenstein's account of the matter is correct. Nevertheless, getting such a discrepancy in counting is a very sure sign in practice that we shall be able to find a mistake, or that if we count again we shall get results that agree. It is because it is such a sure sign in practice that it is possible—or useful—for us to put "$5 + 7 = 12$" in the archives. Thus for Wittgenstein an empirical regularity lies behind a mathematical law.[8] The mathematical law does not *assert* that the regularity obtains, because we do not treat it as we treat an assertion of empirical fact, but as a necessary statement; all the same, what leads us to treat it in this way is the empirical regularity, since it is only because the regularity obtains that the law has a useful application.[9] What the relation is between the regularity and the proof which induces us to put the law in the archives Wittgenstein does not succeed in explaining.

To avoid misunderstanding, I must emphasize that I am not proposing an alternative account of the necessity of mathematical theorems, and I do not know what account should be given. I have merely attempted to give reasons for the natural resistance one feels to Wittgenstein's account, reasons for thinking that it must be wrong. But I believe that whether one accepts Wittgenstein's account or rejects it, one could not after reflecting on it remain content with the standard view which I have called modified conventionalism.

Wittgenstein's constructivism is of a much more extreme kind than that of the intuitionists. For an intuitionist, we may say that every natural number is either prime or composite because we have a method for deciding, for each natural number, whether it is prime or not. Wittgenstein would deny that we have such

[8] III, 44.
[9] E.g., II, 73, 75.

a method. Normally one would say that the sieve of Eratosthenes was such a method; but with a large number one would not —*could* not—use the sieve, but would resort to some more powerful criterion. It will be said that this is a mere practical, not a theoretical, matter, due to the comparative shortness of our lives. But if some fanatic devoted his life to computing, by means of the sieve, the primality of some very large number proved to be prime by more powerful means, and arrived at the conclusion that it was composite, we should not abandon our proof but say that there must be some error in his computations. This shows that we are taking the "advanced" test, and not the sieve, as the *criterion* for primality here: we use the theorem as the standard whereby we judge the computation, and not conversely. The computation is of no use to us because it is not *surveyable*. A mathematical proof, of which computations are a special case, is a proof in virtue of our using it to serve a certain purpose; namely, we put the conclusion or result in the archives, that is, treat it as unassailable and use it as a standard whereby to judge other results. Now something cannot serve this purpose, and hence is not a mathematical proof, unless we are able to exclude the possibility of a mistake's having occurred in it. We must be able to "take in" a proof, and this means that we must be certain of being able to reproduce the *same* proof. We cannot in general *guarantee* that we shall be able to repeat an experiment and get the same result as before. Admittedly, if we get a different result, we shall look for a relevant difference in the conditions of the experiment; but we did not have in advance a clear conception of just what was to count as a relevant difference. (It is not quite clear whether in saying that we must be able to reproduce a proof Wittgenstein means that one must be able to copy from the written proof before one and be certain that one has copied without error, or that one must be able to read the proof and understand it so that one could write it down without referring to the original written proof, so that the possibility of a misprint becomes more or less irrelevant. It does not seem to affect the argument which interpretation is adopted.)

Thus the computation, for a very large number proved prime by other means, of its primality by means of Eratosthenes' sieve

would not be a mathematical proof but an experiment to see whether one could do such enormous computations correctly; for the computation would be unsurveyable in the sense explained. Now what the word "prime" means as applied to large numbers is shown by what we accept as the *criterion* for primality, what we take as the standard whereby to assess claims that a number is prime or is composite. The sense of the word "prime" is not therefore given once for all by the sieve of Eratosthenes. Hence we should have no right to assert that every number is either prime or composite, since for any criterion we may adopt there will be a number so large that the application of the criterion to it will not be surveyable. This throws light on Wittgenstein's insistence that the sense of a mathematical statement is determined by its proof (or disproof),[10] that finding a proof alters the concept. One is inclined to think that such a statement as "There is an odd perfect number" is fixed quite definitely in advance, and that our finding a proof or a disproof cannot alter that already determinate sense. We think this on the ground that we are in possession of a method for determining, for *any* number, whether or not it is odd and whether or not it is perfect. But suppose that the statement were to be proved, say by exhibiting a particular odd perfect number. This number would have to be very large, and it is unthinkable that it should be proved to be perfect by the simple method of computing its factors by means of the sieve and adding them all up. The proof would probably proceed by giving a new method for determining perfection, and this method would then have been adopted as our *criterion* for saying of numbers within this range whether or not they are perfect. Thus the proof determines, for numbers of this size, what the *sense* of the predicate "perfect" is to be.

This constructivism, more severe than any version yet proposed, has been called "strict finitism" by Mr. G. Kreisel and "anthropologism" by Dr. Hao Wang. It was adumbrated by Professor Paul Bernays in his *Sur le platonisme dans les mathématiques*.[11] As presented by Bernays, it would consist in concentrating on practical rather than on theoretical possibility. I have tried to

[10] But cf., e.g., V, 7.

[11] *L'enseignement mathématique*, XXXIV (1935), 52-69.

explain how for Wittgenstein this is not the correct way in which to draw the contrast.

It is a matter of some difficulty to consider just what our mathematics would look like if we adopted this "anthropologistic" standpoint. Would the Peano axioms survive unaltered? "Every number has a successor" would mean, in this mathematics, that if a number is accessible (that is, if we have a notation in which it can be surveyably represented) then its successor is accessible, and this at first seems reasonable. On the other hand, it seems to lead to the conclusion that *every* number is accessible, and it is clear that, whatever notation we have, there will be numbers for which there will not be a surveyable symbol in that notation. The problem seems similar to the Greek problem of the heap: if I have something that is not a heap of sand, I cannot turn it into a heap by adding one grain of sand to it. One might solve the present difficulty by arguing as follows. Let us say that we "get to" a number if we actually write down a surveyable symbol for it. Then we may say: if I get to a number, I can get to its successor. From this it follows that if I *can* get to a number, then it is possible that I can get to its successor; that is, if a number is accessible, then its successor is possibly accessible. Unless we think that "possibly possibly p" implies "possibly p" it does not follow that if a number is accessible, its successor is accessible. We should thus have to adopt a modal logic like S_2 or M which does not contain the law (in Polish notation) "CMMpMp." Another consideration pointing in the same direction is the following. "Surveyable," "accessible," and so forth, are *vague* concepts. It is often profitable to substitute for a vague concept a precise one, but that would be quite out of place here; we do not want to fix on some definite number as the last accessible number, all bigger numbers being definitely inaccessible. Now the vagueness of a vague predicate is ineradicable. Thus "hill" is a vague predicate, in that there is no definite line between hills and mountains. But we could not eliminate this vagueness by introducing a new predicate, say "eminence," to apply to those things which are neither definitely hills nor definitely mountains, since there would still remain things which were neither definitely hills nor definitely eminences, and so ad infinitum. Hence if we

are looking for a logical theory suitable for sentences containing vague predicates, it would be natural to select a modal logic like S2 or M with infinitely many modalities (interpreting the necessity-operator as meaning "definitely"). Thus a suggestion for a propositional calculus appropriate to an anthropologistic mathematics would be one bearing to the modal system M the same relation as intuitionistic propositional calculus bears to S4. (This system would probably have to have axioms of a similar form to those originally given by Heyting, namely, they would frequently be implications whose antecedent was a conjunction, and would have a rule of adjunction as primitive; for, as has been pointed out to me by Mr. E. J. Lemmon, under Tarski's or Gödel's translation an implication whose consequent contains implication reiterated more often than does the antecedent does not usually go over into a valid formula of M, precisely because we do not have in M "CLpLLp.") Another suggestion, made by Dr. Wang, is that anthropologistic logic would coincide with intuitionist, but that the number theory would be weaker.

Wittgenstein uses these ideas to cast doubt upon the significance attached by some philosophers to the reductionist programs of Frege and Russell. We may think that the real meaning of and justification for such an equation as "$5 + 7 = 12$" has been attained if we interpret it as a statement in set theory or in a higher-order predicate calculus; but the fact is that not only the proof but the statement of the proposition in the primitive notation of these theories would be so enormously long as to be quite unsurveyable. It might be replied that we can shorten both the proof and the statement by using defined symbols; but then the definitions play an essential role, whereas for Russell definitions are *mere* abbreviations, so that the real formal statement and formal proof are those in primitive notation. For Wittgenstein notation is not a mere outward covering for a thought which is in itself indifferent to the notation adopted. The proof in primitive notation is not what "really" justifies us in asserting "$5 + 7 = 12$" since we never do write down this proof; if someone were to write it down and obtain the result "$5 + 7 = 11$," we should—appealing to schoolroom addition as a standard—say that he must have made a mistake; we do not even write

down the proof with defined symbols; what, if anything, could be called the justification of "5 + 7 = 12" would be the proof that we actually do carry out that every addition sum "could" be formulated and proved within our formal logical system, and this proof uses methods far more powerful than the rules for ordinary schoolroom addition.

I now revert to the opposing *pictures* used by Platonists and constructivists—the picture of our making discoveries within an already existing mathematical reality and the picture of our constructing mathematics as we go along. Sometimes people —including intuitionists—argue as though it were a matter of first deciding which of these pictures is correct and then drawing conclusions from this decision. But it is clear that these are only pictures, that is, that the dispute as to which is correct must find its substance elsewhere—that such a dispute ought to be capable of being expressed without reference to these pictures. On the other hand, such pictures have an enormous influence over us, and the desire to be able to form an appropriate picture is almost irresistible. If one does not believe in the objectivity of mathematical truth, one cannot accept the Platonist picture. Wittgenstein's main reason for denying the objectivity of mathematical truth is his denial of the objectivity of *proof* in mathematics, his idea that a proof does not *compel* acceptance; and what fits this conception is obviously the picture of our constructing mathematics as we go along. Now suppose that someone disagrees with Wittgenstein over this and holds that a good proof is precisely one which imposes itself upon us, not only in the sense that once we have accepted the proof we use rejection of it as a criterion for not having understood the terms in which it is expressed, but in the sense that it can be put in such a form that no one could reject it without saying something which would have been recognized before the proof was given as going back on what he had previously agreed to. Is such a person bound to adopt the Platonist picture of mathematics? Clearly not; he can accept the objectivity of mathematical proof without having to believe also in the objectivity of mathematical truth. The intuitionists, for example, usually speak as though they believed in the former without believing in the latter. It is true that

A. Heyting, for instance, writes, "As the meaning of a word can never be fixed precisely enough to exclude every possibility of misunderstanding, we can never be mathematically sure that [a] formal system expresses correctly our mathematical thoughts."[12] But intuitionists incline to write as though, while we cannot delimit in advance the realm of all possible intuitionistically valid proofs, still we can be certain for particular proofs given, and particular principles of proof enunciated, that they are intuitionistically correct. That is to say, the point involved here concerns what Wittgenstein calls the motley of mathematics; the question whether a certain statement is provable cannot be given a mathematically definite formulation since we cannot foresee in advance all possible forms of argument that might be used in mathematics. Still, I suppose that someone might deny even this, in the sense that he claimed for some particular logical framework that every theorem that could be proved intuitionistically could be proved within this framework (though perhaps the proof given might not be reproducible within the framework), and yet remain essentially an intuitionist. For the strongest arguments for intuitionism seem to be quite independent of the question of the objectivity of mathematical proof—whether the proof once given compels acceptance, and whether the concept of valid proof can be made precise. The strongest arguments come from the insistence that the general form of explanation of meaning, and hence of the logical operators in particular, is a statement not of the truth-conditions but of the assertibility-conditions. We learn the meaning of the logical operators by being *trained* in their use, and this means being trained to assert complex statements in certain kinds of situation. We cannot, as it were, extract from this training more than was put into it, and unless we are concerned with a class of decidable statements the notions of truth and falsity cannot be used to give a description of the training we receive. Hence a general account of meaning which makes essential use of the notions of truth and falsity (or of any other number of truth-values) is not of the right form for an explanation of meaning.

[12] *Intuitionism, an Introduction* (Amsterdam, 1956), p. 4.

It is clear that considerations of this kind have nothing to do with mathematics in particular, but are of quite general application. They also have a close connection with Wittgenstein's doctrine that the meaning is the use; and I believe that the *Investigations* contains implicitly a rejection of the classical (realist) Frege-*Tractatus* view that the general form of explanation of meaning is a statement of the truth-conditions.[13] This provides a motive for the rejection by Wittgenstein and the intuitionists of the Platonist picture quite independent of any considerations about the non-objective character of mathematical proof and the motley of mathematics. On the other hand, it is not clear that someone such as I have described, who accepted the considerations about meaning but rejected the considerations about proof, would be happy with the usual constructivist picture of our making up our mathematics. After all, the considerations about meaning do not apply only to mathematics but to all discourse; and while they certainly show something mistaken in the realist conception of thought and reality, they surely do not imply outside mathematics the extreme of subjective idealism—that we *create* the world. But it seems that we ought to interpose between the Platonist and the constructivist picture an intermediate picture, say of objects springing into being in response to our probing. We do not *make* the objects but must accept them as we find them (this corresponds to the proof imposing itself on us); but they were not already there for our statements to be true or false of before we carried out the investigations which brought them into being. (This is of course intended only as a picture; but its point is to break what seems to me the false dichotomy between the Platonist and the constructivist pictures which surreptitiously dominates our thinking about the philosophy of mathematics.)

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[13] Cf. also *Remarks*, I, App. I, 6.

DISCUSSION

ON THE SYMMETRY BETWEEN EXPLANATION AND PREDICTION

> . . . An explanation . . . is not complete unless it might as well have functioned as a prediction; if the final event can be derived from the initial conditions and universal hypotheses stated in the explanation, then it might as well have been predicted, before it actually happened, on the basis of a knowledge of the initial conditions and general laws . . . [1]

An example will illustrate Hempel's meaning. In September of 1956 the planet Mars was closer to the earth than it had been in twenty-four years. But besides giving us such a good view, Mars entertained us by coming to a halt beneath the constellation called Pegasus. It then moved "backward," (that is, from east to west). After two weeks of this retrograde motion, the planet again stopped and then proceeded "forward" from west to east.

Let us call this whole complex phenomenon P. What is the explanation of P? A natural way of explaining P is to show how it follows from (E_1), Mars's mean distance from the Sun and from the Earth; (E_2), Mars's mean period of revolution; (E_3), Mars's mean angular velocity; (E_4), Mars's position on Christmas Day, 1955. We must show how P follows from all this (E_1, E_2, E_3, E_4) via the laws of celestial mechanics (which include among others (L_1), Kepler's three laws; (L_2), Galileo's two laws; and (L_3), the laws of Huygens and Newton). Thus P is explained by showing how it follows from E_1, E_2, E_3, E_4, via L_1, L_2, L_3. On this account, however, P could have been *predicted* before September, 1956, simply from the knowledge of E_1, E_2, E_3, E_4, by extrapolation via L, the laws of celestial mechanics. In other words, for Hempel, explaining P is simply predicting P after P has actually occurred. Predicting P is just explaining it before it has occurred. Of course, predictions *per se* are true or false, but not explanations. They are adequate or inadequate. So Hempel's thesis is that the *justification* of a prediction of P is symmetrical with the explanation of P.

[1] Carl Hempel, "The Function of General Laws in History," *Journal of Philosophy*, XXXIX (1942), 35-48; see p. 38.

This is the ideal situation which Hempel describes in the quotation above. In fact, the history of science presents very few examples of disciplines wherein this optimum state of affairs has actually been achieved. Aristotle's cosmology, for example, while it certainly did explain the perturbations of the celestial bodies, could not begin to predict where any planet might appear on any particular day. Nonetheless, his "word-pictures" undoubtedly made the cosmos seem more intelligible to his contemporaries, and in some sense of "explanation" this counts as an explanation. To deny this would be merely to legislate on how "explanation" ought to be used for certain logical or philosophical purposes. It is to leave undiscussed those accounts which actually have counted in the past, and actually count now, as the offering of explanations. Granted that Aristotle's explanation of the cosmos may have been inadequate, it was certainly an explanation; similarly that a prediction does not turn out true does not mean that it was never a prediction at all. But while Aristotle's heavily-ensphered cosmos tendered an explanation of the planetary motions, it could not render up even a *false* prediction. It was just not made for that purpose. Is this not after all much like the claim an historian might make of his account of, say, the decline and fall of the British Empire? He can explain it in just that sense in which it is appropriate to explain historical events. But the historian was not even trying to predict this event, or any other event like it. So what he says cannot, anymore than Aristotle's cosmology, be construed as false prediction.

On the other hand, the great astronomers of the ancient world — Eudoxus, Apollonius, Hipparchus, and Claudius Ptolemy — could to a remarkable degree predict where planets and stars might appear at any future date. But each of them explicitly rules out the possibility of ever explaining the physical principles behind the motions of the cosmos. Theirs was the problem solely of forecasting where familiar points of celestial light would be found on the inverted black bowl of the heavens on some later day.[2] Indeed, the subsequent history of planetary theory could be written as consisting in a conceptual struggle between these two opposed attractive forces, the urge to explain and

[2] ". . . it is no part of the business of an astronomer to know what is by nature suited to a position of rest, and what sort of bodies are apt to move, but he introduces [geometrical] hypotheses under which some bodies remain fixed, while others move, and considers to which hypotheses the phenomena then actually observed in the heaven will correspond . . ." Geminus, quoted in Simplicius, *Commentary on Aristotle's Physics* (ed. by Diels, 1882), pp. 291.21-292.31.

the urge to predict. In some thinkers (Aristotle, for example) the need to explain dominated. Their premature but plausible attempts to give philosophically coherent and intelligible pictures of what lay behind the cosmos resulted in systems which could not be used (even had one tried to do so) for navigation, for agriculture, or for calendrical purposes. For other workers (Ptolemy, for example) these last considerations were not unimportant; indeed they added an incentive to develop a purely predictional astronomy of high reliability, one purged of the unnecessary cosmological overtones which dominated the works of speculative philosophers. Ptolemy was ready to sacrifice a philosophically coherent and unified picture of the heavens above him if instead he could have an accurate instrument of forecast. Through the Middle Ages, through the Merton College astronomers, and Sacrobosco, Peter Apian, Cusa, Oresme, Regiomontanus, Copernicus, and Kepler, one can clearly see these opposed tendencies at work.

Only in Newton's *Principia mathematica philosophiae naturalis* does the final ideal which Hempel outlines seem fully to be realized. The *Principia* does indeed seem to supply us with a single system L which is such that if we can explain any planetary perturbation P in terms of its symbolism and prior facts E_1 — E_4, then we might indeed have predicted that perturbation in terms of that same symbolism and the prior facts. The example of Mars's retrograde motion, with which we began, is elegantly and adequately explained and predicted in the *Principia.*

But, of course, physics and astronomy were soon shot off this happy pinnacle by the discoveries of Leverrier. The very reasoning which led to his (and Adams's) spectacular prediction of the existence of the planet Neptune (if Neptune had not existed, Newtonian mechanics would have been exposed as incapable of explaining the aberrations of Uranus) was employed to deal with the superficially similar precessions of the perihelion of Mercury, a phenomenon also discovered by Leverrier. The "Newtonian habit" had the Frenchman by this time. So he explained Mercury's problems by *inventing* the planet Vulcan. But Leverrier warned us that we should never be able to observe Vulcan, since it was (from Earth's viewpoint) always behind the sun. We know it must exist since Newtonian mechanics is true; but it would not be true in the case of Mercury's perturbations unless Vulcan were there to explain them. This assumption worked perfectly with Uranus and Neptune. The problem here appears to be the same, only Vulcan must be unobservable. This argument did not succeed, however, not

only because of the unobservability condition, but also because Vulcan, the Sun, and the Earth had to be supposed to constitute a straight-line solution to the three-body problem, a solution which is demonstrably unstable. Newtonian mechanics, and physics generally, have never been the same since.

Yet it might still be argued that Hempel has outlined here an ideal situation. It may only have been realized once for a brief moment in the history of science, but that need not matter. But in fact it *does* matter, and perhaps we ought to investigate the situation further.

Hempel's original statement was directed to professional historians. He wished to show that a discipline like history could, from a logical point of view, be put on a basis at least as sound as that of any of the natural sciences. I am not competent to judge the success of this particular effort. Many historians and philosophers of history seem to have found much to disagree with in Hempel's account. But all these critics agree in allowing that Hempel does complete justice to the ideas of explanation and prediction in the physical sciences. Indeed, the examples above suggest that they are right and that Hempel is right.

But it might be argued, as indeed many great physicists (for example, Boltzmann)[3] have argued, that Newton's *Principia* was *from the beginning* an unrealistically ideal scientific system. Of course the ideal situation which Hempel describes is realized in Newton's work, but only because the actually observed phenomena were liberally cooked with unobservables like punctiform masses, invisible forces, absolute space and time, and a host of other mathematically elegant but observationally dubious entities. Perhaps right from the start Newton should have been discussing his data statistically and not geometrically. Perhaps he should have stuck to the absolute letter of "Hypotheses non fingo" — allowing into his physics only those entities which could be observed, operated upon, or experimented with. This would have been an even greater labor than the writing of the *Principia* itself, but then at least his successors would have been in no doubt as to what parts of the Newtonian system are strictly connected with phenomena and what parts are the hypothetical constructions postulated to link the phenomena into a powerful Euclidean-type network of propositions. In short, every L in the *Principia* contains a lot more than the $E_1 \ldots E_n$ which may have suggested it. But we are never clearly told how much more

[3] "Studien über das Gleichgewicht der lebendigen Kraft zwischen bewegten materiellen Punkten," *Sitzb. d. k. Akad. Wien*, LVIII, 8 Oct. 1868; *Ann. Physik*, Leipzig, LIII (1894), p. 959; *Vorlesungen über die Principe der Mechanik*, Leipzig, 1897-1904; cf., also, *Populäre Vorträge*, p. 362.

and we are certainly never given reasons for Newton's unique elaboration of the data: there are, of course, perfectly plausible reasons for his elaborations, but these we must learn from study and inference, not simply from reading his Latin. This, at least, was something like the position argued by Boltzmann after he had written his magnificent work on the theory of gases. Gas theory, and kinetic theory generally, is strictly interpretable on a Newtonian-mechanical basis only if the latter is itself interpreted in a statistical way. And since, like every physicist, Boltzmann wanted one unified physical theory, his only recourse was to re-interpret Newton as a distribution theorist, treating the Ls of the *Principia* as statistical generalizations based on observations rather than as quasi-geometrical axioms based on the physicists' intuition of what is ontologically self-evident.

One could counter here, as Hempel indeed might wish to do, that the limitations imposed on the physicist who is dealing with the phenomena of heat or of gas are observational limitations merely. The statistics enter only because we are, as a mere matter of fact, incapable of dealing with such complex phenomena *en bloc* in simple mechanical terms. The question of principle, however, is in no way affected. Theoretically it remains possible to describe completely the perturbations of any molecule in a gas volume or of any atom in a thermally excited substance. And insofar as this is true, it ought to make sense theoretically to imagine predicting any of the future activities of such molecules and atoms, and thus to imagine *explaining* in terms of such fundamental perturbations all of the more large-scale phenomena which we may in our observations encounter.

If, therefore, anyone were to argue against Hempel that, in gas theory, it is not in fact ever possible to predict the future dispositions of a gas volume's constituent molecules, it could be plausibly countered that this is a limitation due only to the paucity of our information. It is a technical, merely contingent limitation, which in no way alters our basic concepts. There is thus no impossibility involved in the idea of having a complete specification of the internal constitution of a gas at any time. And if there is no impossibility in this, there is no impossibility in talking of predicting the future microstates of such a gas, or in explaining some observed macrostate of that gas in terms of the gas volume's internal microgeometry.

The situation is, however, totally different in quantum physics. Here the facts seem to run wholly against the schemata suggested by Hempel. It is perfectly true that, *given* any single quantum phenomenon P (for example, the emission of a β-particle from a radioactive

substance or the scattering of a γ-ray photon by an electron), P can be completely *explained* ex post facto; one can *understand* fully just what kind of event occurred, in terms of the well-established laws of the composite quantum theory of Jordan, Dirac, Heisenberg, and the later developments of Dyson, Schwinger, and Tomonaga. These laws give the *meaning* of "explaining single micro-events." The philosopher should not legislate here; he must note what *counts* as explanation in microphysics, and then describe it precisely. But it is, of course, the most fundamental feature of these laws that the *prediction* of such a phenomenon P is, as a matter of theoretical principle, quite impossible. Nor is the reason for this impossibility comparable with what obtains in classical gas theory or statistical mechanics. *There* we could not predict because we did not have all the data; still it made sense to speak of having all the data (cf. La Place). *Here*, with quantum phenomena, we cannot predict because we cannot have all the data — or, rather, there exists no clear conception of what it would be like to have data over and above those data with which any well-designed quantum mechanical problem does actually begin. Put more directly, the only theory which explains (in any sense of "explain") quantum phenomena has built into it as a *notational rule of the system* the impossibility of predicting such phenomena. To try to forecast, à la Hempel, the exact co-ordinates of a high-speed electron at some future time would require combining the operators of the formalism in such a way as to violate the very logic governing the defining use of those operators. Part of the syntax of Dirac's quantum theory contains restrictions against predicting such a phenomenon. To attempt the prediction is to move outside the theory, back into classical physics (which explains practically no microphysical observations), or into gibberish.

Now it might be argued that this situation (wherein we find that, although we can explain P, we cannot as a matter of principle predict it) reveals a profound inadequacy in quantum theory itself. This view has been adopted by some illustrious scientists (Einstein, De Broglie, and Schrödinger, to mention a few). Nonetheless, the position is untenable because not one of these thinkers has faced up to an obligation which any complaint against existing theories ought to be obliged to meet.

What precisely *is* the concept we are being asked to entertain when we are asked to entertain a quantum theory fundamentally different from what in fact it now is? What are we being asked to entertain when it is suggested that, although our techniques of observation make it impossible for us to specify the position of a high-speed electron, it

ought nonetheless to make sense theoretically to think of that high-speed electron as having a position beyond the limits of present observation?[4] And what exactly are we being asked to entertain when we are invited to imagine what microphysics will be like when within it the complete predictability of the future state of high speed particles becomes possible? Current theory could not even *describe* such a phenomenon; it would be a totally different theory if it could. (This is what the celebrated von Neumann "proof" establishes.) The answer is that we are being asked to entertain something which is theoretically untenable, empirically false, or logically meaningless. There just is no concept in quantum physics corresponding to the "classical" ideal of Einstein, Schrödinger, De Broglie, Bohm (and Hempel?). The reason for putting it so strongly is as follows. There is now one and only one physical theory dealing with the perplexing phenomena which at the beginning of this century upset classical physics so fundamentally; for example, the black body radiation, the photoelectric effect, the Brownian movement, X-ray diffraction, the Compton effect, electronic, atomic, and molecular diffraction, the Zeeman effect. The classical theory patently cannot do it. It is, however, an essential feature of the new theory that it be expressed in terms of a noncommutative algebra. The position and momentum operators of this physical theory are of the form, *pm-mp = some number*. This is not a superficial blemish on the system; it is the logical structure of the whole theory. In fact, von Neumann once argued that the whole of quantum mechanics can be derived from a suitable observational interpretation of this formula alone. And two of the things immediately entailed by the systematic acceptance of this formula are (1) that there is no intelligible way in this system of speaking of the position of a high-speed electron,[5] and (2) that there is no way in this system of forming predictions of certain types of events (for example, neutron emission from unstable carbon). Yet since this is the only way which we now have of discussing these phenomena at all, and since this theory provides us with the only consistent and empirically applicable concept of elementary particles that there is, the invitation to treat the

[4] As, for example, D. Bohm, "A Suggested Interpretation of the Quantum Theory in Terms of 'Hidden' Variables," *Physical Review*, LXXXV (1952), 166; *Causality and Chance in Modern Physics* (London, 1957); and compare De Broglie, *Compt. Rend.* CLXXXIII (1926), 447 and CLXXXIV (1927), 273.

[5] Ruling out, of course, the degenerate cases discussed by Heisenberg, *The Physical Principles of the Quantum Theory* (Chicago, 1930), p. 20; and H. Margenau, *The Nature of Physical Reality* (New York, 1950), pp. 376-377.

theory as essentially inadequate is equivalent to an invitation to regard our current explanations of all the phenomena cited above as essentially inadequate. Now, while no microphysicist will deny that there is a lot more work to be done with respect to all the phenomena mentioned above — formal difficulties with renormalization and the recent jolt given to physics by Yang and Lee spring immediately to mind — few would deny that we are on roughly the right lines. A quantum-theoretic account of these phenomena does explain to a considerable extent what kind of physical events they are. What is the physicist being asked to do by the person who simply says that there is something wrong with the theory that he uses, but is totally unable to suggest an alternative?[6] Objectors can rarely even point out precisely what is "wrong" with the current theory, other than that it fails to resemble the classical theory in certain obvious respects — for example, the "Hempelian" symmetry between explanation and prediction, which is, I suspect, essentially a consequence of classical determinism.

The situation is much like what astronomers encountered in the middle of the nineteenth century. It was becoming ever more clear that the classical celestial mechanics of Newton's *Principia*, and La Place's *Mécanique céleste* was failing. Many workers could point this out. But so long as no alternative theory was suggested for dealing with complex phenomena connected with, for example, Mercury's perihelion, astronomers did not abandon the concepts they already had; this would have been a counsel of submission to ignorance, an instruction to stop thinking altogether about planetary motion. Similarly today the critics of microphysics have presented no clear idea whatever of what we are to use as conceptual machinery in, for example, elementary particle theory, after we have followed their advice and relegated quantum mechanics to the status of a recognizably imperfect first approximation to some future set of ideas about microphysical nature. What are we to think? What are we to think with?

This is not just an historical remark about the number of theories extant at one time. If someone claims he has a good reason for abandoning theory T, but can suggest no alternative for T for forming

[6] Bohm's is not a genuine alternative. In order to get his modified mechanics to work like a quantum theory at all, he has to take all those formal features of the present system which offend his deterministic sensibilities and bury them in an obscure technical invention of his own, called "quantum potentials." Cf. also Heisenberg, in *Niels Bohr and the Development of Physics* (London, 1955).

concepts about the phenomena in question, I will deny that he has good reason for abandoning T!

All this has deep implications for the general philosophical message in Hempel's thesis. For he puts it that we never really have an explanation of a phenomenon P unless we could, simply by imagining a temporal shift, have predicted P by the same explanatory techniques. For Hempel, therefore, explaining P is simply "predicting" P after it has already happened. But if this is meant to be an account of what is actually *done* in science (not to mention history), where are we to find examples of Hempel's thesis in action? The answer is always the same: in Newtonian mechanics.

That quantum theory is fundamentally nondeterministic is made apparent in the following example: by the principle of identity, every negative electron in the universe must be indistinguishable, save for its co-ordinates, from every other negative electron in the universe. Similarly for every positive electron, proton, anti-proton, neutron, anti-neutron, pion, muon, and so forth. To doubt this principle is to doubt the possibility of physical science altogether, for it is simply the requirement that there should be one basic equation for every *type* of particle, not for every individual particle. As such, the principle is as much a part of classical physics as of modern physics. So there can be nothing, save position, to distinguish, for example, the nuclei of a cluster of atoms of carbon$_{14}$. At any time t, then, all C_{14} nuclei are absolutely identical. But C_{14}, being an unstable isotope of carbon, decays randomly. Fundamental particles are emitted in a wholly unpredictable way (as the uncertainty relations require); certainly it is not the case that *all* the nuclei of the cluster decay at once. But then any particular nucleonic decay must be an "uncaused" event. For if all the nuclei are identical until the time some one of them breaks up, then there can be no causal reason for the decay, since there is nothing to distinguish the context in which the event did occur from the context in which it did not, save the occurrence itself. (Mill's Method of Difference is to the point here.) It is interesting to speculate on what Hempel's analysis of explanation and prediction might have been like had *this* been his paradigm example instead of (as one suspects) those of classical mechanics.

It begins to appear that there is a most intimate connection between Hempel's account of the symmetry between explanation and prediction and the logic of Newton's *Principia*. To have learned this from Hempel is to have learned something very important, both about explanation and prediction, and about Newtonian science. But the historian

need not regard himself as a Newtonian scientist, and the quantum physicist necessarily cannot do so. There may therefore be rather more yet to be said about the logic of explanation and prediction, as these activities actually obtain in fields other than that one in which Hempel's analysis appears to be sound. Because philosophers might still wish to know what is the logical structure of the concepts of explanation and prediction as they are used in living, growing sciences, and not only as they were employed for a brief period in a discipline which is by now little more than a computing device for rocket and missile engineers.

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JUSTIFICATION AND INDUCTION

The Logical Problem of Induction was a doctoral dissertation written by Professor G. H. von Wright for the University of Helsingfors and published in 1941 as a number in the series *Acta philosophica Fennica*. After being some years out of print the book has now been republished in a second, revised edition.[1] The revisions made in the second edition of the book are careful and extensive, some portions having been completely rewritten. In its fundamental structure, however, and in its philosophical position, the book remains unchanged from the original version. Some of the places where changes and refinements of von Wright's views during the intervening years are made apparent are in the discussion of Inductive Logic, one portion of which has been rewritten, the chapter devoted to the Formal Analysis of Inductive Probability, which is likewise a completely new version, and a new section added to the chapter on inductive policies, which offers, as a kind of justification of inductive practice, an attempted analytic proof of the goodness of inductive policies. The full Notes appended to the book have also been revised and supplemented, and are rich in items of information, particularly on the history of various topics discussed in the book, though their location at the end makes reference to them awkward, time-consuming, and unlikely, except by readers who are prepared to follow what is in effect the procedure of reading a book at two places at once. The Bibliography is extensive and useful. Amid all these signs of scholarly industry it is curious that the second edition of the book, like the original one, has been permitted to appear without an index.

I

The topic with which the book is primarily concerned is that of the logical character of the relation that holds between the premises and the conclusion in an inductive inference or argument. How are we to understand the validity, legitimacy, or goodness of such arguments, the manner in which the premises in them give support or warrant to their conclusions? In deductive arguments or inferences

[1] *The Logical Problem of Induction.* Second revised edition. By Georg Henrik von Wright. Oxford, Basil Blackwell; New York, The Macmillan Company, 1957. Pp. xii, 249.

we construe the goodness or validity of a conclusion, given certain premises, in terms of the presence of some kind of truth-guarantee between these premises and the conclusion. So one thing, and the most important thing, that von Wright is interested in determining about inductions is whether in them there can be established to be guarantees of a similar sort. First, there is the question whether the premises in an induction ever provide a proof or guarantee of the conclusion in exactly the way that the premises in a deduction do. This question can be answered, following Hume, with relative ease and negatively; but this is not all. Under the topic of the logical relation between the premises and the conclusion von Wright also includes the question whether the premises in an induction ever provide a proof or guarantee of a different, more complex sort. This is a guarantee that if this particular procedure of drawing conclusions were followed repeatedly, in an indefinitely long series of trials, beginning with true premises in each trial, most of the time, or at least some more or less definite proportion of the time, one would arrive at conclusions that are true.

Answers to these questions are part of what it is important to know about induction, though they are not the only important things. Nor are the guarantees referred to in these questions the only things that might with some propriety be called, in some sense of this term, "justifications" of induction. These questions are substantially the same as those that Hume raised concerning the basis of our inductive beliefs; and guarantees like these, though formulated in a somewhat different way, are the kind Hume supposed it was necessary to provide for inductive procedures if they were to be shown to have a reasonable basis. The questions are therefore referred to by von Wright collectively sometimes as "Hume's problem," and sometimes simply as the problem of the "justification of induction."

The substance of these questions can also be formulated in a somewhat different way. One question one can ask about the various procedures of inductive inference we follow from time to time is the apparently simple one of what grounds we have for supposing conclusions drawn according to this procedure to be true. Suppose one is given a set of premises concerning matters of fact, and a conclusion derivable from these premises by some kind of inductive procedure but itself concerning some matter or matters of fact not reported in or logically entailed by the premises. This is the type of inductive inference, the nondemonstrative or ampliative type, about which the question of grounds or justification has constituted such a prickly,

frustrating problem in modern philosophy. Of course we all naturally trust, when dealing with certain inferences of this type, that we do have good grounds for supposing the conclusions to be true, as Hume remarked a long time ago. But what is in question here is not whether we have psychological attitudes of belief or trust in inductive inferences (pp. 86-88, 138-140, 171);[2] it is whether these psychological attitudes or "states of mind" can be supported by some objective guarantee or proof. Can we justify induction "by substituting for the mere belief that induction will lead to the truth, some guarantees that, under given conditions and with specific limitations, it will *actually* do so" (p. 2)?

A thorough understanding of what is being asked for here is sufficient to provide a complete proof that it cannot be obtained. It is not possible, that is, it is logically impossible, to secure a guarantee of the kind stipulated with respect to any form of ampliative inductive inference. It is logically impossible to establish that, if the same procedure of inference is followed in repeated trials, beginning always with true premises, one will arrive at true conclusions always, usually, or, for that matter, any more or less definite proportion of the time. For this could be established in one or other of only two ways. First, it might be shown that the premises entail the conclusion. This is already ruled out by the supposition that the inference in question is ampliative. Second, it might possibly be established by showing, as it is commonly put, that though the premises do not entail the conclusion, they nevertheless do confer upon it some probability. But in order for a probability warrant, or relation between premises and conclusion, to provide the guarantee sought, the term "probability" will have to be interpreted in what has usually been called a "statistical" or "frequency" sense. Only then would the knowledge that C is highly probable, on the basis of P, constitute not merely some kind of warrant for C, but a guarantee that in the repeated practice of proceeding from true premises like P to conclusions like C, true conclusions will be realized most of the time. This very requirement, that probability must be thought of in a frequency sense, excludes the possibility that the justification or guarantee can be secured along these lines. For when the probability of a given inductive inference is so construed, the claim that a conclusion C is probable on the basis of premises P becomes a statistical matter of fact, a matter of the truth-frequency in the occurrence of certain propositions in the repeated employment of this type of inference. This statistical claim is itself such that it can

[2] All page numbers refer to G. H. von Wright, *op. cit.*

be established only by some form of inductive inference. This means that one inductive inference I_1 can be provided with a justification or guarantee only with the aid of another inductive inference I_2, which will in turn require for its justification another inductive inference I_3, and so on. In short, since the required guarantee or justification of induction can be achieved only by completing an infinite regress of inductive inferences, it cannot be achieved at all.

These are the grounds advanced by von Wright in support of the Humean thesis of the impossibility of "justifying" induction. It will be observed that, as the thesis is Humean, so the argument is likewise closely similar to that advanced by Hume in support of the thesis two hundred years ago. The key points in the argument can also be formulated, as von Wright illustrates, in a somewhat different way which helps to make clear two aspects of the argument: one of these, the central dependence of the argument upon the particular concept of justification adopted; the other, following the adoption of this concept, the logical character of the thesis that inductive conclusions are incapable of being "justified."

The central question asked is whether it is possible, in the case of some types of induction, to provide a guarantee or proof that under certain conditions they will lead to true conclusions, if not in every instance of their employment, at least in some more or less definite proportion of these instances. Is it possible to prove that some form of inductive inference has this property T, which might be called the "truth-yielding" property? The understanding of the thesis of the impossibility of "justifying" induction which is propounded in this book depends upon the recognition that this is the sense in which the key word "justification" is employed in the thesis and in the argument in its support. It likewise depends upon the notice of an unobtrusive but familiar-looking presupposition that plays an important role in this argument. In the context and terminology of this argument the presupposition amounts to this: in the proof that any inductive inference has this truth-yielding property, no similar claim for any other induction can be taken for granted and employed without proof. It is this presupposition in the argument reported above about probability that leads von Wright to the conclusion that this particular route for justifying induction leads to a fatal infinite regress, and from this to the further conclusion that consequently the entire question of the justification of an inductive inference reduces at last to the question whether the realization of this truth-yielding property is logically entailed by the premises of that induction. For the claim

that a given induction has this truth-yielding property must be established, if it is to be established at all, in one of two ways, either a priori or a posteriori. That is, either the proposition that conveys this claim can be derived as a necessary, logical consequence of the premises or, if it cannot, it will have to be established as a synthetic generalization (about inferences with premises and conclusions like these), hence as an inductive generalization, and hence generating a second induction where only one grew before.

This avenue of justification being closed off by the threatening regress, it follows directly that the desired proof, if one is possible, must be of an analytic, deductive kind, demonstrating that possession by the induction of the property in question is entailed by the inductive premises themselves. But this, too, can readily be seen to be incapable of proof, on the strongest grounds, namely, that what is proposed for proof is logically impossible. The request for a justification of the inductive inference has now reduced to a request for a demonstration that the premises of the induction entail, not the conclusion of the induction itself, but the proposition that in the continued practice of inductive inferences of this kind the truth-yielding property in question will be realized. This is of course something that inductive premises, as they are ordinarily conceived, do not do. Nor can they, so long as we continue to mean substantially what we mean now by such terms as "inductive premises," "inductive evidence," and so on. For if the premises of any inductive inference did entail a proposition about the truth-yielding property of that form of inference, it would follow logically that the premises of the inductive inference could not be established as true without establishing the truth of this claim about the inference. This would be a violation of our concept of an inductive premise, making, as it does, the truth of the inductive claim part of the standard of truth of the premise itself. And furthermore, on the presupposition noted above that no such claims can be used in the justification of an inductive inference unless proved, if the premises of any inductive inference did entail this claim, the inference could never be justified, because the premises themselves could never be established.[3]

[3] The argument for the non-entailment by the premises of truth-yielding on the part of the argument is not explicitly given by von Wright as I have presented it, but is substantially a corollary of arguments given by him on other, though closely related, points (cf. pp. 178-182; also pp. 16-22, 29-30, 38-39, 149-153).

II

Are inductive conclusions ever capable of being justified? Or are they always and necessarily incapable of any secure and reasonable justification? The proper answer to these questions is the plain, matter-of-fact one that inductive inferences, like deductive inferences, differ from case to case in this respect. Some are, and some are not, justified and capable of being shown to be so. And any line of philosophical argument purporting to demonstrate that on the contrary this distinction between inductive inferences is an illusory one, that they are all really on the same level in this respect, all hopelessly and necessarily incapable of justification, must somehow be mistaken. Somewhere in the argument for the thesis of the impossibility of justifying inductions there must be a serious logical slip or, more likely, the argument must be rendered possible only because the key term in the thesis, which is "justification," has somehow come to be used in a highly distorted, special, and philosophically illusory sense. In this distorted special sense it may very well be possible to establish the conclusion that all inductive conclusions are unjustified and incapable of justification, just as in a similarly distorted and special sense it has been possible for philosophers to demonstrate the thesis that no one can ever "see" a material object or, in an example closer to the case in hand, that it is impossible ever to "justify" a moral injunction or an ethical evaluation.

Special senses of key philosophical words like "justify," "see," and others are adopted more or less deliberately as expressions and refinements of the senses in which we use such words in the conduct of our nonphilosophical affairs[4]: of the sense, for example, in which we use

[4] Contrary to an opinion now having some acceptance among philosophers, the task of the philosopher in the clarification of our ideas does not end with the elucidation of the senses in which certain words or expressions are used. This conception of function would be incomplete even of the grammarian and lexicographer, and is not less so, in its own way, of the philosopher. One thing constantly disclosed in the careful elucidation of any complex system of concepts is the systematic imperfections of the concepts in their relations to each other, to the situations in which they are applied, and to the purposes for which they are employed, imperfections that in the interest of consistency and completeness call for adjustment or rectification, and hence alteration.

The question about the philosopher is not *whether* he will make these alterations, since this he can confidently be expected to do, but *how* he will make them. Will it be consciously and responsibly, with careful respect for the concepts and usages as given, and a reasoned defense of the emendations

them when we judge that a certain prediction, or a circumstantially based statement of historical fact, is justified or unjustified, or when we wonder whether at a certain moment we actually did see a sail on the horizon, or perhaps were deceived by a trick of our imagination or our eyes. Because they are adopted in this way, because they recommend themselves as expressions and refinements of the senses in which these words are commonly employed, it is a discovery of philosophical importance if it is established that in these new or altered senses these words permit the demonstration of astonishing and even devastating conclusions. In the evaluation of such a discovery one needs to determine two important things: first, whether the establishment of these conclusions is made possible only by the revisions the concepts have undergone in the process of attempted refinement; and second, if so, whether the revisions in question are philosophically reasonable and can be maintained. Otherwise one may be led to think, uncritically and without sufficient grounds, that quite contrary to common sense and the judgment of all those who have not thought about the matter in this particular way, it can be demonstrated by irrefutable logic that, in the senses in which we commonly speak of these matters, or a reasonable revision of these senses, no inductive conclusion ever is or can be justified, and we never do see material objects.

Throughout the argument for the impossibility of justifying induction, von Wright shows himself to be fully aware of the extent to

attempted; or will it be more or less unconsciously, dictated by considerations that, residing in his philosophical unconscious, both permit him to protest honestly that no alterations are intended and prevent his advocating them in a conscious, responsible, and critical way?

With respect to the concepts of cognitive justification such as are here under examination, therefore, a philosopher is not automatically liable to criticism because some differences are revealed between the concepts and usages he endorses and the current, accepted ones with which he begins. He is liable to judgment, to philosophical judgment, whether critical or in approval, on the quality of these differences and the manner in which they are made. It is not a nonphilosophical, but a distinctive philosophical, question whether the alterations are of assistance in promoting a broad understanding of the process of cognitive appraisal and justification, both in themselves and in relation to other major concerns in human life. Do their authors perhaps, in a single-minded respect for the achievements of modern physical science, conceive of justification so narrowly as to confuse and obscure, if not deny outright, the application of such a term in moral, social, and historical matters? Or do they, in a just zeal to avoid so parochial a view, conceive so broadly the range of grounds and considerations that are relevant to judgments of cognitive justification that they lead eventually to the consequence that no judgment of justification can ever be definitively made?

which the argument depends for its validity upon the particular concept of justification employed therein. At several places in the book he takes occasion to observe that if the justification of induction were conceived in some different way, then in one or other of these different ways a "justification" of induction might be achieved along the lines, for example, urged by the classical inductive logicians, or by the conventionalists (e.g., pp. 85, 154). And the chief point of the final chapter, stressed there emphatically, is that the thesis of the impossibility of justifying induction is a logical or "grammatical" one, being "a consequence, not of the constitution of the world but of the use of language" (pp. 177-178, 182); that as such it represents "not a scandal to philosophy, but a philosophical achievement of great importance" (p. 177); and that the view that it does represent such a skeptical scandal is due to a "failure to separate from one another questions of language and questions of fact" (p. 182), for, since the demand for a justification of induction, in the sense intended, is self-contradictory, so correspondingly the thesis of the impossibility of meeting the demand is a tautology (p. 182).

What von Wright does not recognize is the importance of investigating carefully in what way, or ways, it is desirable to conceive of the justification of inductive inferences. What concept, or concepts of justification, judged on broad philosophical grounds, are the most appropriate for evaluating or appraising the "validity" of inductive conclusions? If this question were a purely nominal one, it would of course be easily possible, simply by adopting a concept of justification suited to the purpose, to provide a justification for any induction one wished. Toward the end of his book, in the section added to this edition and entitled "The Goodness of Inductive Policies Considered," von Wright's procedure comes close to this in effect, if not in intention or in so many words. The argument there adduced for the goodness of inductive policies is that an inductive policy for the purposes of generalization and prediction—one that proceeds on the principle that the future will resemble the past— is superior to a noninductive policy for these purposes on two grounds. First, we make the assumption which von Wright regards as difficult to challenge, "that the only things which count as reasons [could conceivably be counted as reasons (p. 173)] for a belief about the future are known facts that are confirmatory of some inductive policy" (p. 175). And, second, having now concluded "that a *reasoned* policy for purposes of prediction and generalization is necessarily equivalent to an *inductive* policy," we see that "if we wish to call reasoned policies *better* than non-reasoned ones, it

follows . . . that induction is of necessity the *best* way of foretelling the future" (p. 174).

In his Introduction von Wright says that if he had written an entirely new book on induction he would probably have made the above point of view concerning the justification of induction a more pervasive one. In some sense of the word, no doubt, it would be correct to call the argument presenting this point of view a "justification" of induction, provided the assumption employed in it were as secure from challenge as von Wright regards it, and provided there is some philosophical ground more substantial than our "wish to call" them so, for affirming that predictive and generalizing policies that are "reasoned," in this particular sense of the term, are better than non-reasoned ones. For once again, the question whether a particular inductive inference is justified is not a purely or primarily nominal question of whether we "wish" to use the word "justified" in a sense in which we should say Yes to this question, or choose instead a sense in which the answer would be No. Rather, like most, if not all, fundamental philosophical questions, it represents a peculiar confluence of two types of question, nominal and real. It is partly nominal, partly a question of the choice and development of concepts, or the uses of linguistic expressions, and partly the thoroughly interrelated question of the relations and application of these concepts or linguistic expressions. Complex issues of both kinds are merged in the question whether —on the broadest relevant grounds, in relation to what we have to take account of scientifically and logically, theoretically and practically, in understanding and assessing the significance of inductive practice—a conception of justification is dictated such that when we approach inductive inferences with an interest in appraising their validity, reliability, or truth-producing capacities, it is correct to say that some inductive inferences are indeed justified.

At the risk of seeming soberly to announce the obvious, I have indicated that the answer to the above question is Yes. That this is so is but a positive way of putting the contention already made that a conception of inductive justification, upon which it follows logically that no induction ever is or can be justified, is a special and distorted conception that cannot philosophically be maintained. The particular conception in question here, and upon which the proof of the impossibility of justification depends, is one which requires, first, that rational confidence in the truth-producing capacities of an inductive inference be supported by a proof that the type of induction under appraisal has the truth-producing capacity in question, and, second, that in

this proof no principles of a synthetic or nonnecessary character be employed unless they too are justified by a similar proof. From the second requirement it follows that the desired proof must be erected solely on the inductive premises themselves; and since this cannot be done, it follows from the first requirement that justification is not possible.

Such, then, is the thesis of the impossibility of justifying induction, the establishment of which may be regarded, in von Wright's words, as a philosophical achievement of great importance. The philosophical achievement in the matter would, however, have been considerably more profound and praiseworthy if those who, like Hume, were most active in this establishment had been more penetrating in understanding the significance of the thesis they were engaged in advancing. For the sober philosophical significance of the establishment of the thesis is not that, in any plausible sense of the term, it is not possible to "justify" any inductive conclusion. It is, rather, that it is not possible to continue to conceive of the justification of induction in this particular way, and that the extreme rationalist aspirations about knowledge that are reflected in it must be given up. Of this point, though the effect is somewhat obscured by the adoption of language calculated to cause a stir among the "learned," Hume's arguments are an elegant and convincing proof.

III

If, in speaking and thinking in philosophy about the justification of induction, we adopt a conception of justification as eccentric as those conceptions employed by Diogenes in his search for an honest man, we should not be astonished to discover, like him, that what we seek is not to be found. But if we wish to speak and think of induction in a way that will have some close correspondence with the ways we speak and think and act with regard to such matters in our non-philosophical pursuits, if we wish our philosophical conceptions to be what might fairly be called rational expressions of these ways, then, guided by Hume's proof, we may begin to discern some of the features of the above conception that are deficient in this respect. There is for one thing the requirement that there be not just a proof that the way of proceeding, in drawing the inductive conclusion, conforms with the standards of reasonable procedure in the pursuit of truth concerning the type of matter at hand, but a proof of the results in the way of true conclusions that will actually be achieved if this way of proceeding is

persistently followed. The frustrations encountered by Peirce in exploring the possibilities of satisfying this requirement are eloquent testimony to both the futility and eccentricity of any such demand. And there is, for another thing, the equally important though less obtrusive requirement that in the justifying proof no nonanalytic or nonlogical principles shall be employed unless they also are fortified by such a proof. This requirement, because in the end it amounts to the demand that the proof shall be effected without dependence upon any nonanalytic or nonlogical principles, might be called "the isolation requirement." To this it now seems desirable to devote a few words of special comment.

The voice is Hume's, and the hands are Hume's, but the spirit is unmistakably Descartes'. When, in the conduct of our affairs, scientific and otherwise, we have occasion to assay the justification of inductive conclusions, we do not proceed, and we have little temptation to try to proceed, in accordance with such a requirement. We do not aspire to judge the reliability or truth-producing capacity of the types of inference employed in isolation from all principles or guides of judgment except those of an analytic or logical kind. Rather, the more we are forced, as we sometimes are, by lack of information or training, to make a judgment in isolation from such principles and guides, the more embarrassed we are by the recognition that what we are called upon to do we are not prepared to do right. Only a confirmed Cartesian, it seems, or someone otherwise ravished by uncontrolled and unreasonable doubts, could, in reasoning about the justification of induction, neglect the primary fact that it is by such means that the assay and justification of inductive conclusions is in practice effected, and insist that it should "in principle" or "philosophically" be effected in isolation from these by the discrimination of some internal logical relation between the premises and the conclusion.

Two things deserve particular notice about these principles or guides of a nonanalytic sort, the use of which in the appraisal of inductive conclusions is a necessary part of any proper and realistic conception of inductive justification. One is the extreme variety of the kinds of guides, whether in the form of principles or not, that can on occasion be used for this purpose. Those having the form of principles may sometimes be established scientific laws, strictly law-like statements of necessary connections. Or they may be formulations of uniformities of a less strict character such as are sometimes called "statistical generalizations." They may sometimes have the character less of substantive principles than of accepted rules of procedure in some cognitive

or practical discipline, like the rules of evidence employed in courts of law or the less precisely formulated canons of historical research. Performing a similar function in other types of inductive investigation are the complex assumptions concerning the nature of the mathematical distribution of the properties or entities under study employed by practiced statisticians in making calculations, according to the mathematical principles of that field, on such matters as probable error and the best tests of hypotheses. And sometimes serving in this capacity, and what is not likely to be scorned by anyone with first-hand experience in any of the arts, sciences, or crafts involving complex inductive skills, is simply the authoritative word of experts in the field whose advice can be used because they are intelligent men who know what are and what are not dependable ways of drawing conclusions about the type of question at hand.

The second thing deserving notice about these nonanalytic guides has already been implied in explaining the first. It is that sometimes these guides are quite sufficient as grounds for effecting the appraisal and justification of inductive conclusions. Whenever it is possible to justify an inductive conclusion at all, it is because, in the context in which the question of justification arises, there are grounds, some of the types of which have been mentioned above, which are in a position to be employed as guides in the answering of this question; and they are in this position because, for one thing, they are part of the context in which the question arises but are not in question themselves. This of course does not exclude the possibility, which it would be fatal to do, that in other contexts the question to be answered might be exactly about the credentials of this guide. When this occurs, and when this new and different question can be answered, new and different guides are employed to evaluate, justify, or discredit the old. The fact that there is no guide that is question-proof, that might not in some conceivable context become itself the subject of appraisal, is a sound point that is recognized in an unsound way in the common allegation that when the guides of inductive justification are viewed as synthetic, the possibility of justification is dissipated in the generation of an infinite regress (cf. pp. 83, 153). It by no means follows that because a guide, whether in the form of a principle or not, can be conceived of as coming into question in *some* context and needing to be proved or disproved, the guide must be so tested and proved before it can be employed in the evaluation of inductive conclusions in *any* context. Such a conclusion is as unsound and as unrealistic as would be the comparable conclusion that because one can always conceive the

occasion arising in which it would be desirable to check, corroborate, or invalidate various types of human testimony, no such testimony can be accepted and employed for any inductive purpose unless it has first undergone such testing and received corroboration.

IV

In these comments on the justification of induction I have emphasized some of the essential aspects of a philosophically realistic conception of such justification and, by contrast, some of the shortcomings in the conception of those who, in the pattern of Hume, construct logical proofs that inductive justification cannot occur. In criticism of the latter type of conception I have urged the unrealistic, in fact fantastic, character of the requirements set in it for inductive justification, in comparison with the norms we try to act by and follow in the conduct and appraisal of actual inductive investigations. Because the limitations of the present essay would have prevented my dealing with it satisfactorily, I have not argued, and will only mention now, what is philosophically the most crushing point against such a conception of justification, namely, that it is at bottom internally inconsistent. That this is so is one of the great lessons to be learned from Kant in his Transcendental Deduction of the Categories. In traditional philosophical terminology the point might be expressed as the necessity of presuppositions, the necessity of them in putting and understanding inductive questions no less than in answering them. Because of this a conception of inductive justification that requires that one assay the justification of an inductive conclusion in isolation from all synthetic principles or considerations except the premises themselves is one that at the same time divests itself of the considerations necessary for understanding what the inference is about, and thus for grasping not only the scope and significance but, therewith, the very meaning of the inductive conclusion that is supposed to be in question.

In contrast with this point about the internal difficulties of the conception of inductive justification, I have chosen to emphasize its somewhat different and external difficulties. I have emphasized the distorted character of this conception, as that is revealed when the conception is contrasted with the very procedures and activities of which it should be a rational expression. In doing this I have in effect been trying to recall to the reader—to adapt the felicitous language of Santayana's *Skepticism and Animal Faith*—not "the principles to which he appeals

when he smiles," but those to which he appeals when he sets out to appraise, justify, or invalidate an inductive conclusion.

It seems reasonable to hope that progress in the understanding of these matters will be accelerated if philosophers of induction can be persuaded to think of inductive justification less as, in accordance with some deep-seated philosophical preconceptions, it appears it *must* be, and more in accordance with the way they and others, acting in conformity with their best logical consciences, do actually proceed to test and judge inductive conclusions. This should help them to see certain things that should not be difficult to see, unless they are obscured by some kind of procedural or conceptual blindness. One thing is that a rational judgment of justification does not require one to attempt to answer all conceivable questions at once. Rational behavior in the pursuit of truth requires this no more than does rational behavior in the conduct of business, the management of foreign affairs, the practice of medicine, or the planning of the family picnic. Similarly, rational decisions on these and other matters are those made in conformity with the norms and guides available and appropriate for such decisions; and questions about the conformity of the decisions with the norms and guides can be and commonly are answered independently of other questions which can on occasion be raised concerning the grounds or justification of the norms and guides themselves.

This is not to be taken as suggesting that decisions about justification are uniformly possible or easy to make. Some inductive conclusions are justified and some are not; and some are incapable of being justified or invalidated, at least at the time the appraisal of them needs to be made, because there is a deficiency in the information or the guides and norms necessary to render a reasonable decision possible. On the other hand, on other occasions, and not uncommonly, such decisions are possible and do get made. The rational justification of inductive conclusions is not logically impossible nor always difficult, and is, in consequence, not even rare.

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A REPLY TO MR. TAYLOR

IN HIS recent article, "The Normative Function of Metaethics,"[1] Mr. Paul Taylor makes a number of criticisms of an earlier paper of mine that was addressed to the same subject.[2] In that paper, I argued that, whatever merits the emotive theory may possess, it is not an accurate description of the way we ordinarily use moral words; and I tried to show how the adoption of an emotivist metaethic would modify the style of the moral life although not necessarily its content. My arguments were not intended to show that the emotive theory must be rejected, but rather that its acceptance should not be urged on the ground that it is descriptively accurate. My primary interest was in the general problem of the relation of second-order analyses to first-order moral discourse, and I used the emotive theory simply as a specimen metaethic to suggest that, in at least one case, the prevailing view of the relationship between these different levels of moral discourse might be mistaken.

Taylor's criticism of my line of argument is twofold. First, he thinks I have confused the emotive theory with subjectivism. He agrees that there are genuine discrepancies between subjectivism and the precritical assumptions of common sense, but he is sure that this is not true of the emotive theory, at least "in its more recent and sophisticated form." Second, even when subjectivism and the earlier, less sophisticated emotivism do run counter to the common sense view, they are not therefore, as Taylor says I tend to suppose, misdescribing the way we actually use moral words but rather contradicting certain "second-order beliefs, assumptions, or views about [one's] own and others' first-order moral discourse," which may very well be erroneous.[3] Taylor thinks that, if I had observed this crucial distinction, I would not have been misled into the belief that we must either reject the emotive theory or find a way of defending it that does not involve the claim that it is descriptively accurate. Taylor's own view is that "we may accept the emotive theory (or whatever 'theory' would constitute a faithful explication of ordinary moral discourse) both as a true descriptive account of ordinary moral discourse and at the same time as an acceptable guide for changing our moral life, including ultimately changing

[1] *Philosophical Review*, LXVII (1958), 16-32.
[2] *Ibid.*, LXV (1956), 159-178.
[3] Taylor, *op. cit.*, p. 17.

that very discourse itself."[4] This statement is not, however, of much help in locating the real issue at stake between Taylor and me, since it consists, on the one hand, of the truistic assertion that any faithful explication of ordinary moral discourse will be a true descriptive account of ordinary moral discourse and, on the other, of a sweeping assurance about the practical implications of such accounts which would be hard to substantiate and is in any case not something my view requires me to affirm or deny. The real issue is whether the emotive theory is or is not descriptively accurate; and while Taylor does not definitely commit himself to the view that it is, he certainly thinks that my arguments do not show that it is not.

The first criticism suggests to me that Taylor's conception of the central theses of the emotive theory is rather different from my own. In my paper, "the emotive theory" meant the views expressed by Professor C. L. Stevenson in his book, *Ethics and Language*, and I think this usage is unexceptionable. It is central to Stevenson's argument that the supporting reasons offered in justification of moral conclusions have no sort of logical compulsion. Stevenson expressly rejects the suggestion that we recognize, in the case of moral reasoning, analogues to validity in deductive and inductive reasoning; and he insists throughout that the nexus between premises and conclusions in a moral argument is psychological or causal in character, not logical. The theory of emotive meaning *explains* these transitions, which are not justifiable by any species of logic. Stevenson does recognize that, unlike "valid," the word "true" does have a distinctive use in moral contexts. When A says, "*x* is good," and B says, "That is true," B is, according to Stevenson, changing the descriptive meaning of A's statement and is talking about his own approval of *x*. In general, although the distinctive function of moral judgment is not to describe attitudes or anything else, it would seem that, for Stevenson, moral judgments can be true only in so far as they describe one's own attitude or those traits of an object that are taken to be criterial for the application of a moral predicate. It is, therefore, natural that in cases of ultimate moral disagreement, where there is no unresolved disagreement in belief, Stevenson is strongly inclined to regard the conflicting moral conclusions as logically compatible with one another.[5]

Taylor's characterization of the emotive theory proceeds along

[4] *Ibid.*, p. 17.

[5] See *Ethics and Language* (New Haven, 1944), pp. 165-173. Also "Moore's Arguments against Certain Forms of Ethical Naturalism" by Stevenson in *The Philosophy of G. E. Moore* (Chicago, 1942), pp. 83-84.

sharply different lines. In its mature form, he says, the theory "purport[s] to show how the rational justification of [moral] assertions is in some respects like, and in other respects is unlike, the rational justification of statements of fact and hence how the words 'true' and 'false' as applied to moral statements differ somewhat but not entirely from their use as applied to statements of fact."[6] It is not clear whether, in Taylor's view, Stevenson's work belongs to the new or to the old dispensation. If to the old, then the simplest way of meeting the objection would be to say that my paper dealt with emotive theory in its early and unsophisticated form. But who would then represent the theory in its mature form? If I had to guess on the basis of Taylor's characterization, I would say writers like S. E. Toulmin or S. Hampshire, but I feel sure that both these philosophers would emphatically repudiate any attempt to identify them as emotivists of whatever degree of sophistication. What Taylor says about the emotive theory would, in fact, be much more appropriate as a description of the logical pluralism, issuing in a claim for the recognition of a logic of moral reasoning, which—to judge by the literature—has been steadily replacing the older, positivistic view of moral discourse as having no distinctive rational structure. It would be decidedly incongruous to associate Stevenson, who explicitly rejects the notion of a third logic and is unwilling to recognize extended senses of validity that would apply to moral arguments, with the philosophers whose practice most nearly conforms to Taylor's prescription and who, as it turns out, have been among his severest critics. No doubt there is some all-inclusive sense in which the term "emotivist" might be made to cover all these very diverse writers, but it would be a sense for which there is little precedent and which would have the unfortunate effect of concealing, under a common rubric, some of the most significant disagreements in contemporary ethical theory. It may be that, as Taylor suggests, common sense is left undisturbed by the conclusions of the "moral reason" school, but I see no justification for assuming, as he does, that the same holds true of the conspicuously different metaethic which is usually referred to as the emotive theory.

Taylor is, of course, quite right in distinguishing between the emotive theory and subjectivism, by which I understand him to mean any theory which makes description of attitudes the primary function of moral judgments. The emotive theory is distinguished from subjectivism by its emphasis on the nondescriptive aspects of meaning and by

[6] Taylor, *op. cit.*, p. 19.

its insistence that the main job of moral discourse is to change attitudes, not just to describe them. But as Taylor himself recognizes, "the truth value of the statement does not depend on this expressive or dynamic function but on the reasons which are given in support of the statement."[7] Now the question of compatibility bears on just that aspect of a judgment in which it is capable of being true or false; and it is, therefore, not apparent what advantage the emotive theory has over subjectivism when it comes to accounting for the fact that we very frequently regard conflicting moral judgments as logically incompatible. If the emotivist is permitted to interpret "*x* is wrong" in such a way that its descriptive meaning changes when it is repeated by a second speaker, then "*x* is right" and "*x* is wrong" will be incompatible when uttered by the same person since he cannot approve and disapprove of the same thing at the same time. As I hope to show, there are good reasons for thinking that this account of the incompatibility of moral judgments is unsatisfactory, but why is the subjectivist debarred from making use of it? He can make "*x* is right" and "*x* is wrong" equivalent, respectively, to "I like *x*" and "I dislike *x*" (or whatever) and these are certainly inconsistent if the "I" designates the same person in both cases. This kind of incompatibility is identical with the kind noted above in the case of the emotive theory; and it would seem that if the latter is proof against the arguments I offered, so is subjectivism.

Taylor's second criticism is more serious. It is not at all easy to maintain a clear-cut distinction between what Taylor calls "the ordinary person's first-order moral discourse" and his "second-order beliefs, assumptions, or views" about the latter; and I am prepared to concede that in my paper it is not always as clear as it might be with which of these the emotive theory is supposed to be in conflict. Taylor himself seems to waver on this point. He admits, for example, that if common sense were to accept the descriptive-evaluative distinction proposed by the emotive theory and "brought it to bear on [the] ordinary use" of certain terms discussed in my paper, "their peculiar force . . . would be lost."[8] Here the change produced by the emotive theory clearly seems to be located at the level of use and not merely at the level of opinion about use. Again, in a passage I have already quoted, Taylor speaks of the emotive theory as a "guide for changing our moral life, including ultimately changing that very discourse itself."[9] The reference is unmistakably to first-order moral discourse and yet it is hard to see

[7] *Ibid.*, p. 18.
[8] *Ibid.*, p. 21.
[9] *Ibid.*, p. 17.

why the latter should be changed by the application to it of a theory which is presumably accepted because it is thought to be a correct description of that very discourse. Nevertheless, the point Taylor raises is an important one and some answer is in order.

I would argue that, to the extent that the distinction between use and opinion about use is capable of being made out, the emotive theory both misdescribes the former and contradicts the latter. To show that it does fail to describe accurately our use of moral words, let us take a typical thesis of the emotive theory and put it to the test. Take a case of ultimate moral disagreement in which A says "*x* is right" and B says "*x* is wrong" without there being any unresolved disagreement in belief that is relevant to the matter in hand. Both A and B are informed of the reasons by which the other justifies his conclusion; neither one challenges any factual assertions that may figure among the reasons given by the other party; and each is convinced of the other's good faith and is therefore prepared to accept the other's conclusion as true in so far as it is a description of that person's attitude. In these circumstances, the emotivist's thesis, stated in its weakest form, is that our use of these moral predicates is such as to be at least consistent with the view that the two judgments are logically compatible. This is what I want to deny.

It should be noted that, if two statements are incompatible, then they are incompatible both when they are made by one speaker and when one is made by one speaker and the other by another. If A were to say "I am hot" and B were to say "I am cold," it would be absurd to regard these two statements as incompatible simply because A could not say what he said *and* repeat B's words, that is, "I am cold," without making one false statement. In order to assess the compatibility of two statements, they must, in effect, be transposed into a form in which their descriptive meaning does not change when the speaker changes. Unless this point is observed, it may be thought that it is an adequate account of the incompatibility of conflicting moral judgments to point out that A, who says "*x* is right," cannot espouse B's judgment without approving and disapproving of *x* at the same time. The trouble with this account is that it establishes a sense in which moral judgments that are made by the same speaker can be logically incompatible, but not one in which such judgments can be incompatible when made by different speakers. This makes it sound as though the logical repercussions of a moral judgment stop short at the boundaries of the ego; and they patently do not. In the situation described above, it would be perfectly normal for A to pass from his assertion that *x* is right to some

such statement as "What B just said is not so." Indeed, A could scarcely refrain from giving some indication that he regards what B said as incompatible with what he said without weakening the force of his own original judgment. The point is that, in our ordinary moral discourse, such statements, by whomever made, have a logical relevance to one another by virtue of which A's statement cancels B's out; and I find it very hard to find a place for this kind of inference within the framework of the emotive theory. What the emotivist does is to split such statements into descriptive and emotive components. In the situation I have imagined, these statements would be logically compatible at the descriptive level; at the emotive level there can be no question of logical relationships between what are really persuasive or minatory gestures. But should there not be some sort of click that would signalize the transition from the bona fide relation of logical incompatibility that obtains between "*x* is right" and "*x* is wrong" when these statements are made by the same speaker to what, according to the emotive theory, is the very different, extra-logical kind of conflict that is supposed to hold between the same statements when one is spoken by A and the other by B in the situation described? If there is, it is an ultra-high frequency click; and if there is such a transition, it is not one that is reflected in our manipulation of such judgments, as one would suppose it ought to be. In fact, a principal advantage of our moral idiom lies precisely in the fact that it permits us to follow out, without hindrance, the implications of our moral judgments over a common and impersonal logical terrain. If there is a legitimate reproach that can be made against the emotive theory, it is that its translations produce a moral idiom that is logically less powerful than the one we currently use and, apparently, need.

Now, it does seem to me what I am discussing is the logic of ordinary moral discourse and not somebody's opinion as to what this logic is.[10] What I have said may sound a good deal like the second-order "assumptions, beliefs, or views" of common sense to which Taylor alludes and it may be mistaken, but what I am talking about is actual linguistic behavior and it can be refuted only by showing that I am wrong about this behavior. This Taylor does not do in his paper.

One further point. In urging that the emotive theory be not judged primarily as an explication of the way we ordinarily use moral words, I have by implication suggested that there are other grounds on which

[10] Taylor would appear to concede this point when he writes that "Olafson shows that common sense has patterned the logic of its moral judgments on a 'cognitivist' basis" (p. 24).

it might be more successfully defended. What are they? I would argue that the inspiration of the theory is, properly speaking, philosophical and that it is a reaction to a conceptual difficulty that was thought, rightly or wrongly, to be inescapable if the language of morals was taken at face value. The concept was that of nonnatural properties and it was felt to be so objectionable, on a number of grounds, that a certain amount of descriptive inaccuracy, to which Stevenson, for example, seems at times to admit, was a small price to pay for its elimination. Viewed in this light, what the emotivists effected was a work of radical conceptual revision in the course of which the concept of nonnatural properties was abandoned and a rigid descriptive-emotive distinction put in its place. Whether the latter was the only genuine alternative is a question I cannot enter upon here, but it is along these lines, and not in the descriptive logic of ordinary moral discourse, that a vindication of the theory might profitably be sought.

There are a number of other points in Taylor's paper on which I should like to comment, in particular his defense of a substantially stronger version of the thesis that metaethics impinges on moral practice than I think is tenable. I do not feel as confident as Taylor apparently does of the socially beneficial consequences of a general acceptance of the emotive theory. Nor, of course, do I subscribe to the widely shared view, which he effectively criticizes, that emotivism and nihilism are kissing kin. But these parts of his paper are not primarily addressed to mine and, therefore, do not require any comment from me.

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BOOK REVIEWS

ETHICS AND THE MORAL LIFE. By BERNARD MAYO. New York, St. Martin's Press, 1958. Pp. viii, 238. $5.00.

This is, in my opinion, a very interesting and stimulating book, one which should be studied by all moral philosophers. It deals with many of the problems which have concerned philosophers recently, and it does so, broadly speaking, in the manner which has come to be associated with Oxford (though it does this only broadly speaking, for Mayo insists that ethics is not a self-contained discipline, as others seem to think) but it goes in some respects rather farther than previous works have done.

In accordance with the present drift of moral philosophy, Mayo conceives of it as an endeavor to get clear what morality is, and hence as concerned with meta-moral questions only, though he believes that its clarifications may be of some help in a practical way. For the understanding of morality he takes as central the notion of principles, rather than that of values or that of virtues, arguing that while such an approach is inadequate it is less so than the others. That is, he conceives of morality as a kind of self-regulation through the use of principles. In working this conception out he pursues two main questions (though he also discusses the more usual topics of the status and truth of moral judgments): (a) What are the characteristics of *moral* versus nonmoral judgments and principles?; and (b) How is it that one's recognition of a moral principle can make a difference to his conduct? I shall present and comment only on Mayo's answers to these two questions and to the related one about the nature of moral authority.

In answering (a), Mayo seems to side with those who seek to characterize morality wholly in formal terms, that is, without assigning it any material content or goal. He appears to regard the following features as individually necessary and collectively sufficient to mark off the moral from the nonmoral. A judgment and a decision are moral if and only if they are made and justified by reference to a moral principle. A principle is moral if and only if (1) it is a principle, that is, is taken to apply not merely to a particular act but to a class of actions; (2) it is taken to apply to everyone's conduct, that is, is universalized; (3) it is assumed that other moral beings accept it, at least hypothetically; (4) it is dynamic or practical, that is, assenting

to it makes a difference to our actions; (5) it is taken as ultimate, unchallengeable, supreme in authority, though not in power; (6) one commits oneself to it by an act of positive assertion in which one is taking sides with others in an at least possible dispute, that is, morality entails a hypothetical dissensus as well as a hypothetical consensus; (7) one commits oneself to it and universalizes it as a member of an organization in which all are autonomous and equal.

In asserting (1) and (2) Mayo aligns himself with such "Kantians" as R. M. Hare and against the existentialists, it seems to me rightly. In insisting on (3), however, he stresses a point which is neglected by such proponents of an individualistic conception of ethics as Hare, P. H. Nowell-Smith, and others, namely, that in a moral judgment one claims an at least hypothetical consensus with others. But he lessens his difference from Hare and the others by what he goes on to say. If we ask what I am to do if I find that the consensus claimed is not forthcoming, Mayo's reply is only that I must change my claim to an invitation (*à la* Stevenson). It would seem more adequate to say that then I must begin to wonder about the tenability of my own judgment. Mayo shies away from saying this, presumably because of his belief that in morality an agent is autonomous. But it seems to me that autonomy in morality does not mean a right to neglect the considered judgments of others, any more than it does in science. Mayo himself implies that one's moral principles may be "wrong," and it is hard to see what this can mean if not that one's principles must be supported by others who review the facts from the moral point of view.

Mayo's most original suggestion is that (6) is not only necessary to morality but is its distinguishing feature. I remain unconvinced, however, that (6) is essential to a moral principle in the same way that (2) is, though I am ready to accept much of what Mayo says about it. As far as I can see, we might have moral principles and enunciate them even if we all knew we were in entire agreement about them, so long as we had some tendency to act contrary to them on occasion. In any case, (5) strikes me as a better candidate than (6) for the office of being *the* distinguishing feature of moral principles, for I take Mayo to be right in holding that moral principles are by definition supreme in authority.

Mayo regards (b) as the primary problem of ethical theory, and treats his discussion of (a) as ancillary to that of (b). His answer, however, is not so clear as it might be, scattered as it is over several chapters. It seems to be that there is a problem only if we misconceive the situation; if, for example, we conceive of it as a problem about

how reason can control appetite or move us to action. In fact, by (4) a moral principle is necessarily one whose acceptance makes a difference to one's conduct. To have a moral principle is to be committed to and disposed to follow a certain rule—to address commands based upon it to oneself. One may still fail to obey it (else it is not a moral principle, either), but one has a kind of readiness to obey it nevertheless. The only question that remains is: how can we acquire such dispositions or readinesses?; and the answer is: by training and practice, as Aristotle saw.

I am not entirely happy about (4), but shall not consider it here, as I have dealt with it in a recent paper.[1] Mayo's analysis of the difficult conception of moral authority is the fullest in recent literature, and it would be interesting to compare it with those of Margaret Macdonald, H. D. Aiken, P. B. Rice, and John Ladd. He insists that morality involves a recognition of some authority but holds, as Hare and Nowell-Smith do, that moral maturity requires that one should be one's own authority. Thus he concludes that "The authority of a moral rule . . . appears in the agent's readiness to obey a command, derived from the rule, and issued by himself to himself" (p. 168). I cannot help feeling that this view of the matter is still excessively individualistic or Protestant, and that one cannot take one's principles as authoritative unless one can reasonably believe that they will be sustained by those who take the moral point of view when they review the facts.

Mayo should be commended for paying some attention to the subject of moral education. I could wish, however, that he had discussed explicitly the important issue between the formalist conception of morality which he seems to espouse and the material conception of it which S. E. Toulmin, Kurt Baier, and others have proposed. For I am not yet satisfied that the latter is mistaken.

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G. E. MOORE: A CRITICAL EXPOSITION. By ALAN R. WHITE. Oxford, Basil Blackwell, 1958. Pp. 226. 25s.

Mr. White's critical account of the work of the late G. E. Moore has many of the virtues that its subject had. It is careful, clear, un-

[1] "Obligation and Motivation in Recent Moral Philosophy," in *Essays in Moral Philosophy*, ed. by A. I. Melden (Seattle, 1958).

pretentious, quite free from verbiage. And it will certainly be useful. When a philosopher is as much discussed as Moore has been, there is always a risk that critics, whether friendly or hostile, will gradually give currency to versions of his opinions that deviate more or less from what he actually said. White is most careful in determining what Moore's own views were from time to time, and his account is supported by very full citation of texts.

He begins by discussing Moore's views about common sense and ordinary language. He inquires in some detail what exactly these are, and in particular he insists, in opposition to a widely held view, that Moore's defense of and appeal to common sense was not just a confused, primitive form of a defense of and appeal to ordinary language. Moore himself perceived these things to be different, as of course they are, and quite properly treated them differently in his writings.

A large part of White's book is concerned next, rightly and inevitably, with Moore's notion of analysis. He traces the course of Moore's repeated efforts to make clear what it is that has to be analyzed and how it is that *knowing the analysis* is not the same thing as *knowing the meaning*. He dissects Moore's different views of what analysis consists in and his different statements of the criteria that a good analysis must satisfy. And he brings out connections between some of Moore's difficulties and dilemmas on this subject and his odd and never fully formulated notions of what it is for an expression to have a meaning.

White then passes to the least rewarding section of his book, in which he discusses Moore's writings on ethics. This section is accurate and clear but it is not very interesting, since White appears to accept without question, and even to carry further, Moore's own extraordinarily narrow conception of the subject. This conception, which practically confines the moral philosopher to discussing the use of such general words as "good," "right," and "ought," has the peculiarity of leaving out—since none of these words is specially moral—almost everything that is distinctive of morality, and of making moral philosophy consist entirely in the scrutiny of a few general concepts which are hardly complex enough to be even theoretically very interesting. It is strange that so many of Moore's successors have followed him along this narrow path.

Quite different is White's discussion of Moore's views on perception. For here he offers, in addition to a good analysis of Moore's views, some fairly fully stated views of his own; and it is perhaps not surprising that this very interesting section is also far more obscure than anything else in the book. His own suggestion (confining himself to the case of

seeing) is that to speak of sense-data is to offer a certain kind—not the commonest kind—of answer to the question "What do you see?". In answering this question we usually *identify* what we see, but we may instead *describe* what we see; and a sense-datum, according to White, is "that which a person would describe if asked to give the *description* answer to 'What do you see?'." The problem of the relation between sense-data and material objects is then, he says, the problem of the relation between describing and identifying; it is a logical not an ontological problem. This looks promising enough. Though White rests very great weight on his distinction between describing and identifying, however, he never makes his use of this distinction clear. In particular, if what he says is to be relevant to sense-data, it seems that he must mean to employ some notion of purely visual, or visually phenomenological, description of what is seen, but he nowhere mentions any such restriction, seeming to rely entirely on the phrase "description of what is seen" by itself. What he says could fairly be taken to *imply* a restriction to a kind of description of what is seen which does not presuppose identification of what is seen. But then what is to count as identification? One may, certainly, identify a bright speck in the sky as an aeroplane; and the "description" here—"I see a bright speck in the sky"—might perhaps be said to be about a sense-datum. But one may also identify a large, long aeroplane as a Comet IV; and one may ask whether, relative to this identification, the phrase "I see a large, long aeroplane" is to be taken as a description—and therefore about a sense-datum—or whether it is to be taken merely to be a less specific identification of what is seen. Where, in fact, does description stop and identification begin? If, as is probably the case, there is no one right answer to this question, then there is, according to White's prescription as it stands, no one right account of sense-data. Yet he ventures to answer with a definite negative such questions as whether sense-data can exist unperceived, or whether they can be otherwise than they appear. There is surely no warrant for his assurance at this point. Can a description of what is seen not be incorrect? And cannot the referent of such a description be there to be seen and described whether or not it in fact is seen or described? The fact is that White has produced little more than a promising program for a new account of sense-data. No definite account can actually be extracted from his words.

His last chapter contains a good short account of some of Moore's younger contemporaries and of their relations to and differences from him. It contains also a mention of certain views of Berkeley and Reid

which resemble, sometimes very closely, some of Moore's views. There is, however, no good reason to believe that Moore was influenced by these writers, and it would have been much more interesting and valuable to have some account of those very different philosophers who were Moore's immediate predecessors, against whom he certainly reacted with great vigor and with effects that have been profoundly important in the recent history of philosophy. Without such an account, Moore's place in that history cannot be understood. It is a great pity that White has nothing to say on this matter.

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MODES OF BEING. By PAUL WEISS. Carbondale, Southern Illinois University Press, 1958. Pp. xi, 617. $10.00.

Professor Weiss's most recent work is a continuation of a series of books all addressed to the problem of "seeing things whole," restoring to unity what the current rage for analysis has split asunder. The author does not, however, regard contemporary empiricism as an opponent, except for its rejection of metaphysics as a legitimate discipline. It is merely one-sided as, in his opinion, all philosophies have been in varying degree. One-sidedness is a natural and excusable fault in creatures whose being is confined to a single one of the four modes, a fault to be corrected only by recognition of the fact that no mode is complete in itself. If empiricism sins in its denial of the being of modes other than that which man possesses (Actuality), other philosophies have sinned in attempting to set up one or another of the other three modes as ultimate realities to the denigration of man's primary mode of being.

The four modes are Actuality, Ideality, Existence, and God. It is not easy to state briefly what Weiss takes a mode to be. All are modes of being but they are not modifications of a substance. Each is "final" yet each is dependent on the other three. It is perhaps most illuminating to call them modes of interpretation or categories, yet they are not psychological or mere forms of thought. Each yields a perspective of the other three, yet no mode seems to be a mere perspective or a position we might take to enjoy a perspective. In one sense any three are demands made by the fourth, that is, requirements for its completion, yet they are not mere demands or hypothetizations but are possessed

of some kind of ontological status. If anything can significantly be called incomplete, then what would complete it must *be*, even though the latter's mode of being is not the same as its mode, else it would already be complete. This seems to be the basic form of argument underlying the distinction between modes and the manner of their interdependence and interaction. The separation between modes is thus employed to account for the fact that reality is not finished, perfect, or completely unified, while the attribution of being to all the modes is used to justify the very meaning of "unfinished," "imperfect," and "incompletely unified."

Actualities are spatio-temporal beings. Ideals are what the name implies: standards, normative possibilities. Existence is most readily equated with energy, self-division, expansion, the dynamic aspect of reality. God is unity, a kind of atemporal, aspatial mode of consciousness in which what is displaced, because separated, in each of the other modes is brought together in wholly significant unity. God, to be sure, performs other functions. He "preserves actualities" by giving them, once they have ceased to be actual (they being mere "affairs of the moment"), the status of fact. Similarly he relates actualities to future possibilities, the latter being incapable without his help of achieving a status in which their relations to present actualities would be pertinent. Nor are God's functions primarily abstract and intellectual. He can be looked to for inspiration, prayed to, sought in mystical experience. Indeed, it is difficult to make out just how close Weiss's God is to the God of conventional religions. But, like each of the other modes, he is not complete, not omnipotent, not in charge of the universe. His perfection is one of his own sort, and he needs the world as much as the world needs him.

Weiss's dialectic is of the kind that frequently leads to the conception of a "block universe," and he frequently finds himself teetering on the edge of that, to him, undesirable position. The incompleteness and separation of the modes is employed to account for contingency, creativity, and moral and aesthetic imperfection. The modes interact, condition, and support one another, and their relations change with time. But Weiss is unwilling to leave all to chance. He seeks assurance, though not certainty, of the preservation of what is valuable. His treatment of possibility is a case in point. A possibility to be genuine must be "real"—that is, *really* possible. But a possibility cannot be real unless it is actualized. This would seem to lead to the "block universe," since only what is actualized was possible and the really possible is what is destined to be actualized. At this point the author

injects a subtle analysis of the law of excluded middle. The latter, "when applicable to facts, and thus to what is present or past, has the form, 'p or not-p'; as applicable to the future, it has the form, 'p-or-not-p.' In the former, the components are distinct and determinate; one of them has been realized and the other excluded as false. In the latter, the components are indeterminate, not yet separable from one another, and thus neither true nor false" (2.13). So future possibilities are not distinct. Actualities, being what they are, limit future possibilities, but do so nonspecifically, and what is actualized, in rendering specific what was nonspecific, apparently does not shear off and render nonreal the vague antecedent possibility. Whether or not this argument is convincing, it is of very considerable importance, for it underlies much of Weiss's reasoning. It seems to be the justification for his interpretation of syllogistic inference as creative (3.33, 3.34; cf. also *Studies in the Philosophy of Charles Sanders Peirce*, Cambridge, Mass., 1952, pp. 166 ff.), and it is involved again in his treatment of normative possibilities, wherein he lays down, as a condition of obligation, that what we ought to do must be "really" possible to us.

The work as a whole is abstruse, chiefly because the reader as he proceeds has continually to revise the meanings he has fixed upon as Weiss's intentions. The author could legitimately reply that this is not a difficulty created by his manner of presentation but one inherent in the structure of being, since no mode of being can be discussed in separation from the other modes, and what one says about one must be adjusted to fit what is said about the others. This may be so, but not enough is said in the beginning about the kinds of difficulty the reader has to expect and surmount. Some advantage might be gained by reading first the last three chapters, in which the relations between the modes are discussed, but it should not be expected that these will be entirely intelligible by themselves. And then, there is a strange interlude between the first and third parts, called "The Negative Route." It takes a very careful reading of the introductory section of this part to make out what Weiss is trying to do in it. Each of the numbered propositions defended in Part I is here restated and refuted. The refutation itself is supposed to fall within the system, thus establishing the positive arguments by a sort of "assertion by denial." Some good things *are* said in the introduction to this part about philosophical polemic and the grounds on which a philosophical system can and cannot be rejected, but I doubt whether many readers will take the trouble to make sure that the set of denials here offered do reassert the system.

It would be most unwise for a reviewer to pass a final judgment on a book of this sort. If it is what it claims to be, it should reward many readings and much thought; and it is, on a first reading, impressive enough to recommend a second. Its claims are likely to be viewed with suspicion in an age when system-building is out of fashion, and they *are* broad. But, as the author remarks, it is of the nature of systems to be inclusive, and of philosophical systems to be inclusive of all that is. The reader is likely to be overcome at times by a feeling that Weiss is merely restating the obvious. Yet the obvious, too, should fall within an inclusive system, and this should be no cause for complaint. More disquieting is Weiss's eagerness to find a place for moral platitudes and political commonplaces. We have grown used to the idea that philosophy should be critical of what almost everyone believes. It is a bit disconcerting to encounter a philosophy which deliberately sets out to justify whole bodies of opinion that have been thought to be internally inconsistent or philosophically unperceptive. Yet this, according to Weiss, is what philosophy should do: "Nothing, the system in effect must show, is wholly wrong, wholly outside it, wholly nonsense. It must show that its opponents' errors are in a way inevitable, as an overinsistence on the need to grasp the system part by part, and thus in opposition to itself as a unity" (p. 381).

Should we ask what of "practical" value might issue from a philosophy of this sort, the answer that comes first to mind is "little or nothing." The book does yield an improved understanding of what the great metaphysicians have been trying to do and why they have differed from one another. Weiss's scholarship is impressive, and insight into meanings and motives of philosophical traditions are not lacking. But perhaps the ultimate "yield" is what, I suspect, Weiss himself was seeking: satisfaction— the satisfaction of knowledge, of the resolution of one's inner conflicts and of the stresses of conflicting claims made by science, religion, and morality. It may be assumed that a different kind of action can be expected from the "whole man" than from the man of fragmented loyalties. If this is so, then man's world, in a more than narrowly psychological and subjective sense of "world," might become different and better because a philosopher has written this sort of book. That would not be an impractical yield. It might, indeed, be the only practical yield of genuine and enduring worth.

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PLATO'S PHILOSOPHY OF MATHEMATICS. By ANDERS WEDBERG. Stockholm, Almqvist & Wiksell, 1955. Pp. 154. Sw. Cr. 19.

The attempt to find in Plato's writings the theory of intermediates attributed to him by Aristotle is likely to remain an "immortal, unageing symptom of our reasoning": its success would not only acquit Aristotle of misrepresentation but would also show Plato to have filled a yawning gap in his account of science. The reviewer must record his opinion, however, that Mr. Wedberg is only the latest casualty in a noble list, so far as concerns this, the main and professed object of his book. It is true that the eternal objects of mathematics in *Republic* 525-527 *might* be taken to be something less than ideas—but only if one could ignore 510d, with its much stronger implication that these objects *are* ideas, and the 500's, whose whole point is that mathematics leads men to distinguish between sensible particulars and *ideas*. Likewise the numbers of pure or philosophical arithmetic, in which "every unit in ten thousand is precisely equal to every other one" (*Philebus* 56e), *might* be mathematical 2's, mere though perfect instances of the property of duality, were it not that at 62a geometry is clearly thought to be part of dialectic and to be the study of the divine circle and the divine sphere themselves, and thus of ideas. If Plato was not a consequential thinker, the intermediates are certainly not in the dialogues; but if he was consequential, they are equally not there. "Below the surface" (Ross's phrase) they can hardly be, since even from there they must have affected his account of the relation between mathematics and dialectic.

The arrangement of Wedberg's book is a parody of scholarship: the text (84 pages out of 154) is supported (a) by notes towards the back of the book, (b) by long appendixes not quite so near the back, to which the notes will refer you, and (c) by notes on the appendixes, right at the back, which will sometimes refer you to another part of the text; one faint-hearted reader has entertained the possibility of an infinite regress. Cross-reference is hindered by the absence of running titles, which would indicate to what chapter, appendix, or set of notes a given page belonged. Annoying misprints occur: on page 118, "pp. 00-00" should be "pp. 64-68"; on page 120, "p. 00" should be "p. 65"; on page 137, "ad III" should be "ad II"; on page 138, "ad IV" should be "ad III"; "groups III and IV" should be "groups II and III." The author deals very inadequately with the previous literature on the subject, particularly with those parts of it (such as Professor Cherniss's works) that reach a conclusion different from his own.

Though the book thus presents (in the reviewer's opinion) a false thesis in a faulty manner, it makes an incidental contribution to its subject that is not insignificant. The author's arguments are very clearly set out, and so honest that he often achieves negative results of some value, as when he allows that the "absolute equals" of *Phaedo* 74b-c may be not intermediate entities but the idea of equality itself, this idea receiving plural expression because a paradigm case of equality must be not one thing but a set of equal things. (Wedberg refers here to similar plurals in *Parmenides* 129a-130a; cf. also P. T. Geach, *Philosophical Review*, LXV [1956], 76.) He uses the mathematical discoveries of Frege and his successors to illuminate the significance of ancient doctrines. Thus Aristotle's view that counting is a type of measurement and implies some measure (cf. *Metaphysics*, 1053a 18 ff.) is interpreted as the recognition that the number of a collection depends essentially on our choice of a unit-concept. Likewise Wedberg compares Plato's recognition of the existence of ideal numbers (supposedly at *Phaedo* 101b-d) with the modern recognition that numbers are not sets of units but (to speak roughly) properties of sets.

This last dogma (that Plato's ideal numbers were meant to be simple entities, not sets of units) has reigned since Cook Wilson's fundamental article (*Classical Review*, XVIII [1904], 247 ff.), usually with no better support than the contention that, since the idea of 2 was the *property* of twoness, it could not consist of two units—a contention that overlooks Plato's distinctive even if mistaken approach to the problem of universals. A paradigm 2 that was a set of units would sort well with ideas (like those mentioned above) that required a plural denomination. (Thus "*dyas*" at *Phaedo* 101c may mean "the couple *par excellence*"; cf. Sextus Empiricus, *Adv. phys.*, ii, 304; also *Cratylus* 432ad, where the ideal number 10 is referred to as "*auta ta deka*.")

Wedberg, reluctant to suppose Aristotle unreliable, dissents from the Cook Wilsonian dogma to the extent of suggesting that Plato came to think of ideal numbers as sets of units at the end of his philosophical development, the paradigmatic view of ideas being a distortion of "the fundamental intuition of the theory of ideas." (Readers may be warned that this involves Wedberg in a chronology of very doubtful consistency, not to mention likelihood.) The flaw in the theory of ideas revealed by the Third Man Argument of the *Parmenides* is traced by Wedberg to a contradiction between what G. Vlastos (*Philosophical Review*, LXIII [1954], 319 ff.) has called the Self-Predication Assumption ("the idea of X is an X") and the Non-Identity Assumption ("the idea is not one among its participants"). That the former of

these assumptions is the cuckoo in the nest is the merest surmise—a surmise, moreover, that Vlastos's article and the discussion to which it has given rise have rendered very questionable.

In regarding Aristotle's testimony concerning the intermediates as unbiased and substantially accurate, the author shows a sound instinct. The distinction between ideas and intermediates neither makes Plato a supporter of Aristotle's own view nor makes him easier to refute, since he was equally wrong (on Aristotle's view) with or without intermediates. The terminology associated with the distinction did not occur in the dialogues, was not that of Aristotle's own theory of mathematics, and cannot have been originated by Speusippus or Xenocrates, since neither of them believed in two types of number or had any motive for reading such a doctrine into the dialogues. It remains then that Plato himself, in conversation or teaching, propounded this distinction to solve certain difficulties and incompletenesses in his theory of mathematics. Wedberg is wrong only in attempting to find it in the dialogues.

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CONCEPTS, THEORIES, AND THE MIND-BODY PROBLEM. Edited by Herbert Feigl, Michael Scriven, and Grover Maxwell. *Minnesota Studies in the Philosophy of Science*, Volume II. Minneapolis, University of Minnesota Press, 1958. Pp. xv, 553. $7.00.

This is the second volume of papers produced under the aegis of the Minnesota Center for the Philosophy of Science. The editors describe them as the fruits of "collaborative research," and the authors obviously have been energetically discussing their problems with one another. Each paper is independent, however, and the authors do not always take account of the criticisms voiced by their collaborators. Roughly speaking, most of the authors fall into two groups: Oppenheim and Putnam, Hempel, Pap, and Feigl tend to choose and to treat their problems in the manner of traditional logical empiricism, making considerable use of formal logic; Scriven, Strawson, and Alexander more or less represent contemporary British philosophical analysis. Although much of the writing is infelicitous in style and much of the philosophical thinking pedestrian, the volume is worthwhile in that it makes available useful statements of varied views.

The earlier papers in the volume discuss the nature of theories, of dispositional concepts, and of causation. In "Unity of Science as a Working Hypothesis" Oppenheim and Putnam ask whether all the terms and all the laws of science are reducible to the terms and laws of some one discipline (say, subatomic physics). Whether there is "unity of science" in this sense they take to be an *empirical* question, and they maintain that there is. Unity of science would be demonstrated, they say, by "microreduction"—completely explaining aggregate phenomena in terms of their parts, these parts in terms of their parts, and so on, until we reach an ultimate level (they speak of the "certainly true empirical assumption that there does not exist an infinite descending chain of proper parts"—a curious bit of empirical certainty). Unfortunately the authors do not do very much to show that unity of science really is an empirical thesis, for they do not sufficiently make clear that their thesis could conceivably be false. They do say that what they oppose is the doctrine that some phenomena are emergent, absolutely irreducible; but it is not clear what sense that doctrine makes. Thus one is left wondering whether the claim that there is unity of science, if true, isn't perhaps a trivial necessary truth.

Carl G. Hempel in "The Theoretician's Dilemma" is concerned with the role in science of "theoretical terms"; these he thinks of as terms whose applicability in a given situation can never be decided merely by direct observation and which cannot be explicitly defined by means of observational terms either. It has often been maintained that such terms are themselves uninterpreted yet can aid explanation and prediction by providing connections among observables. But the dilemma is this: if the theoretical terms and general principles of a scientific theory serve their purpose of establishing definite connections among observable phenomena, then (as Craig has proved in general) they can be dispensed with in favor of laws which directly link observational antecedents to observational consequents. So why use theoretical terms at all? Hempel's answer is that to eliminate theoretical terms by Craig's method would cause scientific theories to be much more cumbersome and unmanageable than they are when formulated with theoretical terms. It would cause a loss in "heuristic fertility and suggestiveness," and it would cause theories to lend themselves less well to the formulation of inductive predictions. He thinks that this is justification enough for employing theoretical terms, and he goes so far as to suggest that one could regard "partially interpreted" theories as actually true and having "factual reference." Thus Hempel attempts to resolve his dilemma while yet retaining the idea that there is a sharp

distinction to be drawn between observational and theoretical terms. His paper is a valuable one, for it presents clearly the puzzle to which this distinction gives rise; he does not do much, however, to show that the distinction itself is soundly drawn, and one who suspects that it is not may feel that the dilemma merits dissolution rather than resolution.

A quite different point of view is expressed by Michael Scriven in a rambling paper, "Definitions, Explanations, and Theories." He discusses the porosity of actual language and brings out that explicit, formal definitions very often are pointless. Then he attacks as unclear the reductionist thesis (maintained by Oppenheim and Putnam) that the terms of one branch of science can be defined by means of the terms of another branch. Scriven declares that "the reductionist thesis, in so far as it involves definability is untenable, in so far as it involves redefinability is pointless, and in so far as it involves anything else amounts to the assertion that there's an explanation for everything." Scriven attacks also the deductive conception of explanation (favored by Hempel), maintaining that typical scientific explanations are essentially nondeductive in character, because the laws we use for explaining do not hold without exception but are statistical.

Arthur Pap in "Disposition Concepts and Extensional Logic" argues that the dispositional statement "sugar is soluble" would be true even if there never were any liquids in the universe at all. For this somewhat naive reason he rejects all attempts to define dispositional predicates by means of devices of extensional logic. He declares instead that in order to express them we must have some sort of "natural implication" as a primitive indefinable notion ("This is sugar and is put into water *naturally implies* that it soon dissolves"). And what of Hume? Pap answers that this is "perfectly consistent with Hume's theory of causation." Ah, these indefinables, how slickly and soothingly they lull us asleep! But Hume's sharp wit, one suspects, would not have been soothed by this.

The topic of causation is treated also by Wilfred Sellars in "Counterfactuals, Dispositions, and the Causal Modalities," the least easy to read paper in the volume. Sellars does not help one to decipher his views when he declares, "To take the causal modalities at their face value, that is to say, to interpret statements concerning what is physically necessary or possible or impossible as belonging to the object language of scientific (and everyday) discourse, which statements, however intimately they may be related to such metalinguistic statements as they may, *in some sense* imply, are nevertheless not themselves 'really' metalinguistic, is certainly to court serious philosophical per-

plexity." But at any rate, his view of dispositional and causal statements (which he, too, declares to be compatible with Hume's) would appear to be this: he claims that to establish by induction that A and B are causally connected is to decide to adopt the new rule of inference that "B" may be inferred from "A," thus altering the meanings of "A" and "B" so that "if A then B" becomes analytic. This extraordinary view has at least the merit of novelty.

The reasons which have led philosophers like Sellars to describe causal laws and other types of general statements as rules of inference next are attacked in "General Statements as Rules of Inference?" by H. Gavin Alexander. He grants that we often do make causal inferences without stating any explicit major premise, but he denies that this requires us to describe the causal law as a rule rather than a proposition. He denies that the logic of induction is in any way clarified by the claim that laws are rules, that the claim illuminates actual scientific practice, or that it helps us to understand subjunctive conditional statements. He refreshingly concludes that there is very little to be said in favor of the notion that general statements should be construed as material rules of inference, inference licenses, or inference tickets.

The remaining papers in the volume mainly deal with the philosophy of the mind. P. F. Strawson in "Persons" treats the concept of something which is *both* a subject of experiences and a part of the material world. He points out that it makes sense for one to apply to oneself predicates implying consciousness only if one also applies such predicates to other persons; and one can do that only if one can recognize from the behavior of others that these predicates apply to them (and one cannot be reasoning merely by analogy here). Thus it is fundamental to the concept of person that one can have an "adequate basis" for applying such predicates to another simply by observing his behavior; yet also that one can have "adequate basis" for applying them to oneself without observing one's own behavior. It is perhaps a pity that Strawson does not bring out perspicuously what he means by "adequate basis"; but his paper is a useful one, though of course its main ideas are far from being wholly new.

Herbert Feigl in a long, rambling paper, "The 'Mental' and the 'Physical,'" shows that he has not been influenced by Strawson or by those who influenced Strawson. Feigl deals with the old mind-body problem: how are "raw feels" related to neurophysiological states? He thinks of this as an empirical question, to which a conclusive answer can be given someday when future physiologists develop an instrument which will enable an observer to watch the insides of his

own brain and compare their alterations with the alterations in his "raw feels." Feigl tentatively argues for the identification of "raw feels" with neural events: when I am acquainted with a sensation in my mind, he maintains, the sensation *is* an event in my brain. That this is so is an empirical theory, he claims; it is not a necessary truth, but is the simplest explanation. Taking for granted that his "raw feels" are things, and that it could make sense to identify them with material things, Feigl doggedly finds nothing incongruous about the idea that we see, hear, feel, smell, and taste the states of our brains.

Karl Zener in "Experience of the Individual" attacks behaviorism and defends "direct experience" as a notion useful to psychology. Paul Meehl in "When Shall We Use Our Heads Instead of the Formula?" succeeds in illustrating that frequency theories of probability fail to account for judgments about single cases. And the volume concludes with an appendix containing a correspondence between Wilfred Sellars and Roderick Chisholm on "Intentionality and the Mental"; both are in favor of intentionality, but they debate whether it is more fundamentally a feature of thoughts or of words.

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AUTHORITY. Edited by Carl J. Friedrich for the American Society for Political and Legal Philosophy. *Nomos*, I. Cambridge, Mass. Harvard University Press, 1958. Pp. viii, 234. $5.00.

This volume should be particularly interesting to scholars in jurisprudence, political science, sociology and, among philosophers, to those who are mainly concerned with moral, social, legal, and political philosophy. The contributors are Charles W. Hendel, Carl J. Friedrich, Herbert J. Spiro, Jerome Hall, Frank H. Knight, Hannah Arendt, Norman Jacobson, George E. Gordon Catlin, Wolfgang H. Kraus, Bertrand de Jouvenel, David Easton, Talcott Parsons, and E. Adamson Hoebel.

The essays are grouped under three headings: historical, general, and socio-political. Because most of those who are likely to read this review will be philosophers, I shall confine it to the several analyses of "authority." From this restricted perspective, the most pertinent and valuable papers are those by Friedrich, de Jouvenel, Easton, and Hall. All of them agree that the test of the analysis should be the adequacy of the concept of authority that it provides for a systematic theory of

human relations. This is the correct criterion of evaluation, and its self-conscious adoption lends the essays a high degree of sophistication.

Friedrich is not clear as to the exact connection between "X has authority" and "X has the capacity to back up his communications (assertions, biddings, commands, requests) with reasons worthy of acceptance in the light of X's own and the audience's shared opinions, beliefs and values." There are statements in his essay which would be affirmative answers to these questions. Is the connection one of equivalence in meaning? Is the longer expression only a logically necessary condition of the shorter one? Is the connection, whichever of these two it is, stipulated by Friedrich? Is it an elucidation of the familiar use of an expression? Is the shorter expression no different from "X is an authority"? I hazard a guess that, of these incompatible alternatives, Friedrich would commit himself to these: The connection is that the longer expression is a logically necessary condition only. The analysis is stipulative in the sense that, familiar usage aside, Friedrich will simply refuse to call anything authority if it does not satisfy his logical criterion. Finally, "X has authority" is no different in meaning from "X is an authority."

I am not altogether happy to be forced to criticize a view which I can at best attribute to its author by guesswork. If my guess here is correct, Friedrich's analysis is vulnerable. First, not all communications can satisfy Friedrich's criterion. Imperatives—for example, "Shut the door"—and normatives—for example, "You ought to shut the door"—are alike in two respects. Both tell us what to do or not to do, and both may be construed as answers to the question: "What should I do? What action is required of me?" But there is an important difference as well. It is no part of the meaning of an imperative that there are reasons worthy of acceptance to back up the imperative. If you issue the imperative "Shut the door," I may ask, "Why should I?" You may reply, "For no reason at all. I am just telling you to do it." This answer of yours in no way entitles me to say that you do not understand imperative locutions. But except in the case of primitive normatives, the same answer would entitle me to say that you do not quite understand the way in which normative locutions are used.

Imagine an inarticulate sergeant. He commands his squad in battle with consistent skill. The men obey him blindly. By Friedrich's analysis, the sergeant is not exercising authority over the men in the squad because he has no "capacity for reasoned elaboration" of his commands. But then, how are we to classify the sergeant's relation to the men? It is not one of sheer power. The men could shoot him

if they wanted to. It is a special kind of power, the kind that has all the earmarks of authority. Or imagine a mother telling her child to go see if the laundry in the backyard is dry. She does not really care to know. All she wants is to get the child out of the room for a little while without letting it know this. The child does as asked. Again, by Friedrich's analysis, the mother has not exercised authority over the child. She has no reasons worthy of acceptance in the light of the beliefs, opinions, and values she and the child share to back up the imperative. Such reasons as she might have will back up a different bit of communication, namely, "I want you to leave the room for a little while." But as in the previous case, how are we to classify this relationship? It is not one of coercion or sheer force or brute power. It is a special kind of power—the power to move to action by means of an imperative where the effectiveness of the imperative need not be a function of implied or explicit physical coercion. It is quite natural to call this kind of power "authority." And the communications involved in the relationship do not logically require "reasoned elaboration" by those who issue them, either as a condition of their intelligibility or as a condition of their effectiveness.

Next, it is a mistake to assimilate "X has authority" to "X is an authority." Robert Oppenheimer is an authority in matters of nuclear physics. He had no authority in relation to the A.E.C. in matters pertaining to the social and political implementations of physical knowledge. Edward Teller enjoyed both kinds of authority. It is because of this illegitimate assimilation that Friedrich finds it plausible to bring scientific and political authority under his concept.

Last, we can quarrel with stipulations only on grounds of explanatory value. The range of phenomena Friedrich explains under his concept can be explained under some other concept. And there are some phenomena, including the case of the sergeant and the mother, which Friedrich arbitrarily excludes from coming under the concept of authority.

De Jouvenel's and Easton's analyses are more promising. On page 168, de Jouvenel refers to his forthcoming book *Of Sovereignty: An Inquiry into the Political Good* (Chicago University Press). De Jouvenel contends that his concept of authority is the basic explanatory concept of a general theory of human relations, especially political ones. Until we see the book we will not know how good the claim is. But judging from the essay in *Authority*, I should take it seriously. De Jouvenel defines "authority" in terms of the *efficient imperative*. "An agent A formulates an imperative statement; thereupon the action indicated

in the statement is performed by an agent B or a set of Bs. Compliance of the Bs establishes for the observer an empirical relation, namely, that for the complying Bs, A's statement has proved an efficient imperative. If we find this process regularly repeated between the same agents, we shall feel inclined to say that A has a certain power to move the Bs, or again that he enjoys a certain authority over them." This is *sui generis*. In particular, it is not reducible to the bargaining relation.

There is but one point which I find dubious in de Jouvenel's discussion. In disagreement with the Hobbesians, he holds that an association logically cannot arise out of an encounter of wills. But his proof of this, if not invalid, is inconclusive. This is his argument:

1. There *must* (my italics) be an individual who takes the initiative in founding an association.

2. The duration, growth, and success of the body generate in its members' loyalty toward it.

3. Finally, the means which are placed at the disposal of the body's ruler allow him to reward or punish the performance of the members.

Therefore, *logically* (my italics), this comes last (not first as Hobbes and his disciples believe).

The conclusion is a logical consequence of the premises. But the temporal relation mentioned in the conclusion is not logical. It is factual because it follows from factual premises. The argument is invalid if the word "logically" is intended to characterize the temporal relation stated in the conclusion. Besides, this way of reading the conclusion makes it sound as though the Hobbesian thesis is a logical one, and obviously it is not. Furthermore, even if we read the word "logically" as indicating the deducibility of the conclusion from the premises, the argument is inconclusive. The word "must" in the first premise is too strong. It is not logically impossible that an association should arise out of the initial confluence of several "wills" coequal at the outset. An association may so arise, and this is consistent with the Hobbesian social contract theory.

De Jouvenel may be misled by an ambiguity. On page 161, he holds that the Hobbesians cannot explain "whence the means to threaten originated at the outset of political relations." In the third premise of the argument he speaks of "the means which are placed at the disposal of the body's ruler to reward or punish the performance of members." Such rewards and punishments logically cannot occur in a state of nature because they are, by definition, institutional. But threats can occur. In the city jungles, simulacra of Hobbes's state of nature, any neighborhood tough can organize, by threats, a small gang which may

then mushroom into a sizable business operation. The means of the tough do not have to originate anywhere. The means are the toughness itself.

Easton regards authority as a special kind of power relationship. "A has exercised authority over B" means "Under conditions that (1) A intends B to do X and (2) B is aware of A's intention, A has the capacity, by bidding B to do X, to get B to do X without B's evaluating the doing of X in terms of his own standards of what is desirable under the circumstances." This analysis agrees with de Jouvenel's in several important respects. It distinguishes authority from the bargaining relation; it makes essential use of imperative or otherwise action-inciting locutions; it regards authority as a relation between the formulator of an efficient imperative and the doer of the action specified in the imperative; and, unlike Friedrich, it construes the relation of communications and the reasons one might give in their support as not universally a logical relation. The other refinements, which are important for distinguishing force, persuasion, manipulation, and authority, are easily assimilable into de Jouvenel's primitive concept of authority.

I have no fault to find with Easton's analysis. His concept appears to have the earmarks of a workable scientific tool. But again, its full evaluation must wait upon a book-length application.

Hall's essay calls attention to the different ways in which authority crops up in legal contexts. The examples he supplies are part of the data which must be accounted for in any analysis of "authority" if it is to pass the test of conceptual adequacy.

Friedrich rightly calls attention to the "rational element" in many authority relations. His fault is serious but corrigible. He would have all action inciting authoritative communications *logically* ride on the back of reasons worthy of acceptance. As a generally adequate analysis this fails. Its proper emphasis on reasons in certain types of communication can be easily accommodated by the Easton-de Jouvenel type of analysis. They simply distinguish authoritative communications which are backed up by reasons from those which are not. There is no reason why they should not further admit the distinction I outlined above between normatives and imperatives. They can accordingly distinguish one class of authoritative communications—the normatives—all of which, except the basic ones, logically ride on the back of reasons, from the imperatives, the understanding of which is not a function of the understanding that reasons are available to back them up.

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PRACTICAL REASON AND MORALITY. A Study of Immanuel Kant's Foundations for the Metaphysics of Morals. By A. R. C. DUNCAN. Toronto, Thomas Nelson and Sons, Ltd., 1957. Pp. 182. $4.00.

Professor Duncan has written a valuable book but he has not proved what he wished to prove. He has asked a neglected question and given a challenging answer. The question is: What sort of book is the *Grundlegung?* He considers two answers:[1] (a) It is a fundamental part of a metaphysics of morals; its title should be *Foundations of the Metaphysics of Morals*, where the preposition "of" indicates that the foundations (fundamental practical principles) belong in the body of ethics. (b) It is a partial critique of practical reason and propaedeutic to metaphysics of morals; its title should be *Foundations for the Metaphysics of Morals*, where the preposition "for" indicates that the fundamental principles are theoretical principles about the essential character of morality. Answer (a) is called "the Ethical interpretation," (b) "the Critical interpretation." Assuming that the two interpretations are as incompatible as the programmatic answers make them seem, the author decides for the latter.

Like many others, he came early to the conclusion that Kant's ethical theory, as apparently expounded in the *Foundations*, is indefensible. But he followed the sound principle that when a great philosopher sounds silly, the fault may lie in the reader. Instead, however, of adopting the procedure of others who accept this principle and try to show that the ethical views in the book are not silly, he took the revolutionary course of saying that there is no ethical system here at all, and that it does not matter if the ethical views expressed in it from time to time are in fact wrong. They were incidental to Kant's purpose, and a great part of them was an interpolation which confuses the reader about the purpose of an otherwise straightforward argument.

The crucial question is the interpretation of Section II, "Transition from the Popular Moral Philosophy to the Metaphysics of Morals." What Duncan calls Subsection B, paragraphs 12 to 32, gives a general description of practical reason as a preparation for the transition proper, in Subsection C, paragraphs 33 to 75. C restates the categorical imperative, derived in B, in terms drawn from the popular moral philosophy expounded in Garve's commentary on Cicero. C is ethical

[1] Actually Duncan considers three, but one is quickly disposed of as wrong. I would say, on the contrary, that, properly interpreted, it is right but a part of the other two. For brevity I omit discussion of it.

but is not a structural member of the book, being an interpolation irrelevant to it as a partial critique. Thus Duncan.

There are serious flaws in this ingenious hypothesis. Kant does not clearly say that B is preliminary to the transition. The sentence at the end of paragraph 11, on which everything depends, says that in order to make the transition we must follow the practical faculty of reason from its universal rules of determination to the point where the concept of duty arises. Duncan reads this to mean that we must *first* carry through this inquiry, with the result that the transition itself is deferred to C. Now this means that that which gives the title to the whole chapter is found in an irrelevant interpolation. This is startling enough to make us re-examine the sentence in paragraph 11 to see if it can be given a less paradoxical meaning. It can. "Do X in order to do Y" does not necessarily mean "Do X so that you can subsequently do Y"; it may mean, and it means here, "The doing of Y includes the doing of X." The study of practical reason contains the transition, not a preparation for it. C is a corollary or application of the principle derived in B, with the purpose of bringing this principle "closer to intuition." If C were the transition, it would be a transition in the wrong direction, from metaphysics to popular moral philosophy, and not the reverse.

Now why did not Duncan see the transition in B? I can suggest two reasons. (1) He fails to appreciate the fundamental distinction between practical and pure practical reason. *Passim* he writes "practical reason" where he ought to write "pure practical reason" in order to conform to Kant's exposition; it is not surprising that in a passage were Kant only tacitly uses the distinction his critic should miss it altogether. The last sentence in paragraph 11 means that to make the transition we have to examine practical reason up to the point where the claim of pure practical reason is announced. All popular philosophers had recognized that reason could be practical; none had shown that pure reason could be practical. But when the concept of duty is reached, Kant shows that it must come from pure practical reason. At that point popular moral philosophy gives way to metaphysics of morals in the sense of a body of a priori practical principles. C, therefore, instead of presenting the popular moral philosophy or a transition from it to metaphysics, itself gives the fundamentals of the metaphysics of morals.

(2) Duncan thinks that when Kant says, in paragraph 12, that a rational being acts in accordance with the conception of laws, he is thinking only of moral laws. "He therefore conceives of rational beings

as acting not in accordance with laws of nature, but in accordance with a special kind of law, a law of freedom or a moral law" (p. 103). This is incorrect in two respects and leads to the greater error of mislocating the transition. Of course rational beings act in accordance with laws of nature; if they did not, there would be no problem of freedom. What is distinctive about them is that they act also in accordance with their conception of laws. But what laws? Duncan thinks moral laws. Kant thinks they can act in accordance with their conception of natural laws and that they do so under all hypothetical imperatives. These laws, however, do not morally necessitate. For duty, another kind of law is required which can be conceived only by a pure practical reason. Hence the laws of nature must be supplemented by another kind of law conceived a priori and belonging, therefore, to the metaphysics of morals. The transition to the metaphysics of morals from the popular philosophy occurs where the conception of laws of nature, so dear to the popular philosophers, is replaced by the conception of an a priori law of pure practical reason.

There is much that I would like to praise in this stimulating book and much else I would like to criticize. Had Duncan attempted merely to bring a "partial critique of practical reason" to the full light of day but had not attempted to replace the not incompatible conception of it as a foundation *of* the metaphysics of morals, I would have commented upon quite other points, intrinsically more interesting, in the positive development of his own "Critical" interpretation.

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THE PHILOSOPHY OF KARL JASPERS. The Library of Living Philosophers, Vol. IX. Edited by PAUL A. SCHILPP, assisted by LUDWIG B. LEFEBRE. New York, Tudor Publishing Company, 1957. Pp. xxvi, 918. $10.75.

Although the name of Karl Jaspers is well known in this country, most of his works, including the bulkiest, have not yet been translated into English from the rather forbidding German in which they are written. Consequently, this latest volume in an impressive series of interrogations of living philosophers may well serve the valuable function, in addition to its principal aim, of introducing many American readers to the thought of Jaspers. In fact, a number of the contributions

are mainly expository in character. Thus, those interested in helpful expositions of such central notions as "the Encompassing" (and its modes), "ultimate situation," and the importance of science in Jaspers' philosophy will be amply rewarded by such essays as those of Kurt Hoffman, Edwin Latzel, Gerhard Knauss, and James Collins. Likewise, an essay of chiefly critical intent, "The Psychology of Karl Jaspers" by Ludwig B. Lefebre, includes an excellent exposition of its subject; and an essay by Hannah Arendt presents a clear and concise appreciation of Jaspers' notion of "the pivotal age" which should arouse the interest of historians. Not all of the twenty-four essays are written with equal clarity, but the number of passages whose obscurities seem insuperable is not excessive considering what is par for the existentialist course to date.

The themes of the volume are the Big Questions: death, freedom, guilt, authenticity, the place of the artist, God, world history, evil. For Jaspers, philosophy is not an *ancilla scientiarum* but an *ancilla vitae*. To philosophize is "to go beyond the intellect without losing one's head" (p. 794). While audacious, he is also modest: "Shall we give up the basic operation just because we are not immediately successful?" (p. 793). He does not believe that the narrowing of philosophy, as traditionally conceived, to Exceedingly Small Questions is even thinkable: "Such resignation and renunciation are not possible even for one who is willing. For whoever rejects philosophizing, unavoidably practices philosophy after all; although he does not know it, he practices an unnoticed, bad philosophy" (pp. 793-794). On the other hand, opponents have sometimes charged that existentialism narrows philosophy by developing a dramatized version ("ultimate situations," for example) of personality theory and abnormal psychology. The issue would seem to be a significant one, and Jaspers speaks to the point emphatically in his reply to Lefebre (pp. 811-817).

The philosophical mind revealed by Jaspers' two contributions (a philosophical autobiography and a reply to his critics) and implied by the critical essays belies the popular belief in a particular existentialist temperament. In comparison to Jean-Paul Sartre, for example, so adept at dispatching an opponent in a few swift sallies, Jaspers seems more measured, circumspect, and tentative. As compared to Martin Heidegger, whose thoughts point steadily toward one overwhelming problem of metaphysics or "pre-metaphysics," the mind of Jaspers is far less lean and solitary, far more responsive both to the patchwork of current events and to the variety of traditional philosophical problems. If one had to name a counterpart in American philosophy of equal diversity

and vitality, one might well think of John Dewey. Where Dewey has spoken firmly, however, Jaspers has spoken more dramatically and even elusively, perhaps in keeping with his keener awareness, Nietzschean and Kierkegaardian, of the mystery and depth of human experience. Kurt Hoffman's essay, "The Basic Concepts of Jaspers' Philosophy" (pp. 95-113), shows that Jaspers is by no means unsystematic; at the same time, Jaspers professes no system as such. Thus, his replies to critics are often generous in underscoring the truth which may be in their objections, except when the objection is that his philosophy should be less suggestive in the Socratic mode and more clearly debatable in the mode of a Descartes or a Hume. Regarding the manner of philosophical communication, he is adamant: he would have us, so to speak, listen with the third ear. It is perhaps this driving concern of Jaspers' existentialism with the problem of human communication on fundamental issues (cf. Fritz Kaufmann, "Karl Jaspers and a Philosophy of Communication," pp. 209-295) that gives to the composite of essays, autobiography, and reply its unmistakable air of contemporary relevance. For the existentialist, however, this is not in the first instance a problem of social and technological organization, as for John Dewey in *The Public and its Problems*, or of linguistic purification, as for our more semantically oriented philosophers, but a matter of achieving that openness to Being which renders men free, undogmatic, and authentic individuals. To many ears, such a dictum may sound impractical and obscure, particularly when uttered out of context. Before passing judgment, non-existentialists should, of course, examine the details of Jaspers' philosophy which purport to give significance to such ideas. To this end, this volume should be a great help to English-speaking philosophers.

All the same, one can hardly help wishing that this work devoted to a philosopher during his lifetime and so rich in expository value were no less rich in criticism from non-existentialists, in the give-and-take which Jaspers himself, by all accounts, prizes so highly. This scarcity of really radical interrogation of Jaspers is undoubtedly a sign of the times, not an editorial failure. After all, a sheet of paper inscribed "Nonsense!" from an American or British philosopher of analytic bent would hardly be a usable contribution, and in our age of expertise it is improbable that anything more sober and informed could be expected. While we are looking a gift horse in the mouth, it should be added, of course, that the existentialists, for their part, are hardly famous for any serious consideration of contemporary modes of philosophy outside the existentialist circle. Disagreements in this volume, then, tend to be

family quarrels, occurring within the existentialist style of thinking and speaking. Yet, page after page would seem to suggest topics of probable interest to existentialist and non-existentialist alike. For example, there is in Jaspers' conception of values, and even in his conception of truth, what in our journals would be termed a sort of emotivism. This is a position congenial enough to American and British thought. However, whereas all that lies outside empirical or logical demonstration tends for us to coalesce indistinguishably into the merely emotive, into that which is *not* further discussable, Jaspers finds in the same region numerous distinctions which he believes to be of capital importance for science no less than for morality, art, and religion. Another topic of probable mutual concern is the question of one science or many sciences. Jaspers argues for pluralism, a position defended (on different grounds, to be sure) by many American thinkers. Also, there is in Jaspers' philosophy a keen sense for important methodological issues, as there is in epistemology in England and here. These remarks are not intended to depreciate the intrinsic value of the intramural criticism, which has its own appeal and significance. It is only to say—particularly in view of the German existentialist's concern with communication—that it would be exciting indeed if our times permitted a serious and respectful interchange between, so to say, Being as the Encompassing and being as the value of a variable. But then, an editor is bound to remember with misgivings the obtuse lampoon of Martin Heidegger written by no less distinguished a thinker than Rudolf Carnap. Communication among solitaries, as the philosophy of Jaspers stresses (cf. p. 213), unquestionably remains a problem of contemporary philosophy.

The most notable exception to primarily intramural criticism is the essay entitled "Anthropology in the Philosophy of Karl Jaspers," in which William Earle directly challenges Jaspers' account of Existenz-clarification and goes on to claim that existential insight is in a genuine sense cognitive and therefore objectively communicable. One detects here the attempt of an essentially rationalistic position to admit the contents of existential philosophy within a different framework and conception of evidence. That this is precisely the type of challenge which should be issued and answered through The Library of Living Philosophers is proved by Jaspers' own comment: "I confess that for me the decision between the being and not-being of philosophy lies precisely at this point" (p. 820). Another radical challenge is issued by Hans Kunz, who argues that the fact of death can be faced without that sense of a Transcendence which for Jaspers is the necessary corre-

late of Existenz. If Earle is right concerning the objectivity of existential awareness, Jaspers' entire edifice might crumble, and if Kunz is right concerning the rounding off of our little lives, a mainstay is removed.

From the editorial standpoint, the only flaw is that some translations from the German retain too many of the characteristics of German syntax and style. As a suggestion for future volumes, it might be worth considering the addition of a separate table of contents for the reply to the critics in order to facilitate cross-reference between objections and answers. To the enormous credit of The Library of Living Philosophers is the completion of what surely must have been one of the most taxing editorial jobs of recent publishing history, a task which included extensive annotation, a fourteen-page bibliography of Jaspers' writings, a useful glossary of Jaspersian terms, and a strenuous effort to render the basic terminology uniform throughout the variety of contributions. For carrying this heroic achievement through to completion, Editor Paul A. Schilpp and his helper, Ludwig B. Lefebre, deserve the highest praise.

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PHYSICAL SCIENCE AND PHYSICAL REALITY. By LOUIS O. KATSOFF. The Hague, Martinus Nijhoff, 1957. Pp. viii, 311. Guilders 17.75.

This book attempts to cover a very wide range of topics in the philosophy of physical science. Chapter headings are: "Introduction to the Philosophy of Physical Science"; "Science as a Language"; "What Physical Science Talks About"; "The Nature of Explanation"; "The Nature of Scientific Statements, Laws"; "The Origination and Confirmation of Laws: The Principle of Induction"; "Truth and Probability of Laws"; "Causal and Non-Causal Laws"; "The Nature of Hypotheses"; "Confirmation by Experimentation"; "The Structure of Theories"; "The Vocabulary of Physical Science"; "Meaning of Scientific Terms"; "The Semantics of 'Space'"; "The Semantics of 'Time'"; "Relativity—Motion; Matter—Quantum Theory"; "Causality"; "Presuppositions of Science"; "Physics, Reality, and Perception." This is a lot for about three hundred pages, and it is not surprising that the treatment is frequently perfunctory and on rather an elementary level. The book would therefore appear to me more likely to be of use as an introductory textbook than as a treatise for advanced students, but I would myself

hesitate to recommend it to elementary students because it contains rather a lot of errors, and the thinking is frequently rather loose.

Most of the actual errors are admittedly somewhat trivial but they do make comprehension difficult. For example, it is not true in a society in which bigamy is allowed that, given any two distinct women, there is at least one man such that each woman is the wife of that man (p. 14). The definition of "true" near the bottom of page 20 could be very misleading. On page 21 there should have been some mention of rules of inference. On page 60 it is said to be analytic that all persons have hearts. On page 63 it is said that the subjunctive conditional does not *easily* (my italics) translate into a disjunction. The diagram on page 74 is incorrectly drawn. In the discussion of Coulomb's law on page 76 the connection between the fact that the time of oscillation of the needle is as the square of its distance from the pole of the long magnet and the inverse square law of attraction should have been explained. On page 78 it is said that in deduction we are given a set of rules which can be applied *mechanically* (my italics) to obtain a desired conclusion, if it can be obtained. Similarly, near the bottom of page 79. At the top of page 80 it is suggested, without supporting reason, that we can assign a numerical probability to a law. In the second paragraph of page 88 there is a glaring inverted-comma fallacy. The use of "other facts" near the bottom of page 90 is very unclear. There is queer talk on page 105 of absolute temperature being *caused* by kinetic energy, and of force being *caused* by a moving mass. On pages 134-135 the theory of the Michelson-Morley experiment is incorrectly worked out. There seems to be a use-mention confusion on page 160 where it is said, "whether the existential assertion affirms the existence of particles, or energy, or quanta, or waves, or mathematical expressions. . .". Chapter XIII seems to suggest a "Fido"—Fido theory of meaning: for example, page 191, "When we ask for the meaning of a scientific term, we seek to find out what it stands for 'and' even constructs stand for something." On page 204 it is said that the Lorentz equations imply that if a measuring rod moves in one direction it shrinks, but that if it moves in the opposite direction it expands. The use of "non-Euclidian" on page 212 could be misleading.

The book is, however, written in a fairly pleasant prose style, without overmuch jargon and without pomposity. The general philosophical position, with its opposition to operationism and phenomenalism and its realistic attitude to theoretical entities, is one with which I am very much in sympathy. The latter half of Chapter VI makes some useful points on confirmation. There is a suggestive point on page 158,

where it is held that a theory is rejected when doubt is thrown on the existence of entities, for example atoms, mentioned in it, whereas if we merely alter some of the laws relating to these entities we have not given up the theory. (But this account is clearly applicable only to some types of theory.) There are also such good points as those on page 209: "the difference between the being there to be measured of a relation like 'distance from' and of a body like a table or meson"; on page 247: " 'electron' does not function as does 'matrix' or 'integral' or 'number'"; and page 249: "'electrons are mathematical' simply means 'electrons obey mathematical laws.'" There is good stuff in Chapter XI on the structure of theories, and also in Chapter XV on the Semantics of "Time," though on page 221 there seems to be an over-operationist notion of "separation" which makes Katsoff want to reduce separation of events in time to the separation of bodies in space by a material rod. What about the theoretical notion of separation given, say, by Minkowski? There are useful bibliographical references at the end of each chapter, though there is one missing where it is definitely needed, namely, for the reference to Black on page 84. There are a good many misprints. "Heisenberg" is consistently misspelt.

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MARXISM AND THE OPEN MIND. By JOHN LEWIS. New York, Paine-Whitman Publishers, 1957. Pp. xviii, 222. $5.00.

Dr. Lewis is a Communist philosopher who has put English-speaking students of Marxism-Leninism in his debt by his translation of Shirokov's *Textbook of Marxist Philosophy*. He has also written an *Introduction to Philosophy* which has been translated into Arabic and Japanese. The present book is a collection of articles and lectures on subjects ranging from the philosophy of Sartre to the Marxist view of ethics and of human rights, this last article being Lewis's contribution to the Symposium prepared by UNESCO for the Human Rights Commission of the United Nations in 1947. Lewis disagrees with "the philistine approach" of Zhdanov who claimed Lenin's authority in condemning all bourgeois philosophy out of hand, and the book's rather simple secret may be discerned on page 184 where the author, in the course of a sympathetic account of Sartre's political beliefs, says that "no Marxist movement can live without the support of intellectuals, who bring it prestige, self-confidence, and clarity of theoretical understanding."

The most interesting essay in the book is that entitled "Marxism and Ethics." In this Lewis tries to rebut the accusation that Marxists think that a communist social order must be good because they believe it to be inevitable and thus subordinate moral convictions to the march of history. He seems to think that it helps his argument to say that moral standards "evolved within history," but by means of this phrase he surely confuses the truism that moral principles relate to human actions in historical circumstances with the absurdity that what will be is necessarily what ought to be. According to Lewis, "social utility is the sole basis of moral principles." From this he infers that "there is no moral rule that duty may not compel us to break in exceptional cases." He quite rightly points out that if this utilitarian view is accepted, then it is not admissible to condemn acts of coercion or deceit on the sole ground that they conflict with moral principles. He fails to notice, however, that if there are different ways of doing good, then it is likely that human happiness can be produced in different types of social order, and not only in the communist one. In a lecture entitled "Historical Inevitability," Lewis claims to "reply" to Popper's and Berlin's criticisms of historicism, but quotes so little of the detail of their arguments that he makes it appear that he has nothing to reply to. In the chapter "On Human Rights" he emphasizes the importance of upholding the liberal tradition of free speech and criticism and says that restrictions on this can be removed "when people have become accustomed to the new ways of thought and there is little or no danger of reversions to anti-social ways." This is a bland, apologetic book, devoid of any intellectual incisiveness. It demonstrates that a Communist can write with good humor, but not that he can analyze the basic notions of his creed.

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WORDS AND IMAGES. A Study in Theological Discourse. By E. L. Mascall. New York, The Ronald Press Company, 1957. Pp. xi, 132. $3.50.

The study of theological discourse will not be materially advanced by this book. In large part it is taken up with attacks on "empiricism." First there are criticisms of empiricist treatments of theology, ranging from the A. J. Ayer of *Language, Truth, and Logic*, through the discussion on "Theology and Falsification" in *New Essays in Philosophical Theology*,

edited by Anthony Flew and A. C. Macintyre, to Braithwaite's recent Eddington Memorial Lecture. In his criticisms of the last Mascall is rather acute, but his flogging of Ayer is based on *very* well worn points (e.g., the status of the verification principle itself) which hardly need restatement at this hour of the day. And the section on the Theology and Falsification discussion is little more than an extended summary. Mascall also makes a more general attack on Empiricism by urging the Thomist position that "perception is primarily an *intellectual act*, an act in which the mind utilises the phenomenon as an *objectum quo* and passes through it to grasp the concrete trans-sensible reality, whose nature is that not of sensible but of intelligible being. I asserted that only in this way was it possible to maintain, not only that we can come to know and to speak about God as the creative ground of the universe, but also that we can come to know and speak about physical objects and persons other than our own selves" (p. 121). And against "the ideal of knowledge as detached, discursive and arrested at the level of phenomena" he sets "another ideal of knowledge as involving commitment, contemplation and penetration beneath the phenomenal level," and claims that "it is only by such an approach to them that the realities with which Christianity is concerned can be discovered and explored" (p. 122). The author is to be commended for insisting on the relevance of epistemological considerations to the analysis of theological statements. But he has not made clear, to this reader at least, the significance of such a dark saying as "its [the physical world's] contents are intelligible beings which are not sensed by the senses but apprehended by the intellect" (p. 35). After reading the book I am still at a loss as to how I might determine whether my *intellect* is or is not *apprehending* something when I look at a tree. Moreover, Mascall betrays no awareness that the phenomenalist brand of empiricism he is attacking has already been severely attacked from within (what he would call) the empiricist camp, both in the physicalist branch of logical positivism and in the writings of such men as Wittgenstein and Gilbert Ryle. Nor does he take account of the possibility that a nonphenomenalist version of empiricism might equally give rise to just those doubts about theology which he is seeking to allay.

When the announced topic, theological discourse, is finally taken up in the last chapter, the results are disappointing. After more polemic, this time against "communication-engineering," misinterpreted as assuming a structural isomorphism between sentences and thoughts, it is suggested that the key to the interpretation of theological discourse is to be sought in the doctrine of analogy and in a consideration of the

crucial position of images. But the treatment of these topics consists of tantalizing hints, supplemented by suggestions for outside reading in the works of Austin Farrer and E. L. Mascall, among others. The subtitle given the book, "A Study in Theological Discourse," is, to say the least, misleading.

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POETIC DISCOURSE. By ISABEL HUNGERLAND. University of California Publications in Philosophy, Volume 33. Berkeley, University of California Press, 1958. Pp. iv, 177. $3.00.

The title of this able essay indicates its point of view, one which it shares with much of the New Criticism and the New Philosophy of our time. It holds it to be self-evident, or at least beyond the need of inquiring into alternatives, that poetry and literature are to be considered as "a special kind of discourse," and that the primary task of the inquirer into poetry is to distinguish it from other kinds of discourse, as well as to discriminate the varieties of discourse exhibited in different poems and in different places in a single poem. The ruling supposition is that the traditional problems in describing and appraising poems will be solved or dissolved when we come clearly to understand how language works.

While not uncritical of some commonplaces of recent philosophical analysis, Mrs. Hungerland herself, for the most part, skilfully applies to poetry the concepts and devices of the Oxford and Cambridge analysts. She is less set on giving firm answers than on disentangling profitable from profitless questions and on mapping out the procedures for posing and answering these questions intelligently. She likes to appeal to ordinary language, not as invariably setting the criteria of sense and nonsense, but as a clue to distinctions, ambiguities, and conditions of usage which, when overlooked or mistaken, lead to confusions between categories, false analogies, and a failure to detect the diverse questions which may lurk within a single interrogation. And she distrusts generic statements about "art," "literature," "poetry," "aesthetic experience," and "artistic unity"; for such generalizations invite the "formula approach" and the essence-fallacy, and disguise the fact that "particular cases" constitute not a homogeneous class but a family of diverse individuals, with indefinite extension.

The author employs such concepts and procedures to illuminate many of the standard topics in poetry and aesthetics. She demonstrates tellingly the radical deficiency of the cognitive-emotive dichotomy, when this is taken to dispose adequately of all uses of language; she discriminates various aspects of figurative language, while exposing some common errors or half-truths about their role in poetry; and she clarifies the use of "intention" and other kinds of evidence for deciding among alternative "interpretations" of a poem. Her longest, and on the whole her best, chapter is devoted to the problems of literary appraisal and to supporting the claim that the "core criteria" of aesthetic value are, in an important sense, "objective." Mrs. Hungerland displays a salutary skepticism about ambiguities, ironies, archetypes, symbols, and other favorite shibboleths of contemporary criticism. On the other hand, she exhibits a virtue not always evident in philosophers who have otherwise profited from Wittgenstein but are as quick to cry "Muddle!" as any militant logical positivist is (or used to be) to cry "Nonsense!" For Mrs. Hungerland is tolerant of other modes of philosophical verbalizing than her own; nor does she assume that if a statement is not all right, it must be all wrong. Her characteristic procedure is to search out the condition of use or modicum of valid observation which, even if inadequate as the basis for a position, explains why it has seemed reasonable to an intelligent inquirer.

By demonstrating so well the capabilities of linguistic analysis when applied to literature, this book defines the more clearly its limitations. Mrs. Hungerland rightly insists that "poetry cannot be marked off from other kinds of discourse in terms of some specific kind of meaning, feature, or function of language predominant in it" (p. 43; cf. pp. 7, 13). On what grounds, then, does she herself confidently identify as exemplary poems or works of literature such instances as *Hamlet*, Jane Austen's *Emma*, Browning's *My Last Duchess*, Marvell's "To His Coy Mistress," and Yeats's "Sailing to Byzantium"? And repeatedly, in "trying to place poetry among the uses of discourse," she refers to certain features of poems which she calls "characters," "action," and "plot." But how did these terms get into her discussion? For to describe Hamlet, Browning's Duke, and the lyric speaker of "Sailing to Byzantium" as "modes of discourse," or to define the action of dramatic or novelistic narratives as "a use of language," is to use language in a way which is as inept and unpromising as in any of the instances to which Mrs. Hungerland properly objects.

Roughly speaking, to analyze literature solely with the concepts of

the "forms," "functions," and diverse "conditions" of linguistic usage will enable us to recover and clarify the matters discussed in Aristotle's *Rhetoric*, but not the matters discussed in Aristotle's *Poetics*. A number of the features discriminated in the *Poetics*, however, seem indispensable to anything like an adequate description or analysis of poems. Following this or that critic in the past, we may prefer to substitute for Aristotle's identifying characteristic of "imitation" other terms—"feigning," "invention," "making," or "presentation." But what is imitated, invented, made, or presented is not a mode of language. Instead, language is used to put before us one or more speakers and agents who, in various settings and circumstances, utter their thoughts, describe their perceptions, convey their feelings, purposes, and characters, and perform the actions that constitute the plot. And though they will disagree on borderline cases, most experienced readers will agree in identifying as poems or works of literature those paradigm-cases in which, whatever the overlap with other human products, the distinctive feature is that the characters and what they say or do are presented primarily for their inherent interest, as elements in an ordered whole which we tend to regard as valuable in itself.

Poetic Discourse manages best with those topics for which a linguistic analysis is most nearly adequate. Its limitations appear when it takes up matters which cannot be encompassed either by ignoring the extra-linguistic features of poems, or else by smuggling them in, as needed, in defiance of its own premises. Mrs. Hungerland vigorously addresses herself to the literary problems of "truth," "verisimilitude," "artistic unity," "the relevance of moral values to aesthetic or artistic values," and the difference between "aesthetic" and "real-life emotions." Such problems, however, will not yield very far to an inquiry which does not, in some fashion, recognize the serious and adult "let's pretend" of literary invention, and which, limited on principle to linguistic considerations, cannot allow for the presentation in literature of human beings and human experiences, very like those in real life, although so rendered as to be intrinsically interesting, moving, and delightful, and so arranged as to make a work sufficient, and complete.

M. H. Abrams

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